Birkhäuser

Giorgio Israel
Ana Millán Gasca

The World as a Mathematical Game

John von Neumann and Twentieth Century Science

Science Networks
Historical Studies
Volume 38

BIRKHÄUSER

Science Networks . Historical Studies
Founded by Erwin Hiebert and Hans Wußing
Volume 38

Giorgio Israel
Ana Millán Gasca

The World as a Mathematical Game

John von Neumann and Twentieth Century Science

Birkhäuser
Basel · Boston · Berlin

Authors:
Giorgio Israel
Dipartimento di Matematica
Università di Roma “La Sapienza”
P.le A. Moro, 5
00185 Roma
Italy
e-mail: giorgio.israel@uniroma1.it

Ana Millán Gasca
Dipartimento di Matematica
Università Roma Tre
Largo S. Leonardo Murialdo, 1
00146 Roma
Italy
e-mail: ana.millan@uniroma3.it

Translated from the Italian by Ian McGilvray

2000 Mathematical Subject Classification: 01A60, 01A70, 03-03, 68-03, 93-03

Library of Congress Control Number: 2008941508

Bibliographic information published by Die Deutsche Bibliothek
Die Deutsche Bibliothek lists this publication in the Deutsche Nationalbibliografie;
detailed bibliographic data is available in the Internet at http://dnb.ddb.de

ISBN 978-3-7643-9895-8 Birkhäuser Verlag AG, Basel - Boston - Berlin

Basel · Boston · Berlin
P.O. Box 133, CH-4010 Basel, Switzerland
Part of Springer Science+Business Media
Printed on acid-free paper produced from chlorine-free pulp. TCF ∞
Cover illustration: with the friendly permission by Marina von Neumann Whitman
Printed in Germany

ISBN 978-3-7643-9895-8 e-ISBN 978-3-7643-9896-5

9 8 7 6 5 4 3 2 1 www.birkhauser.ch

Contents

Abbreviations vii

Introduction ix

1 János Neumann's Early Years
1.1 A Jewish family in early twentieth century Budapest 1
1.2 A young talent in Hungarian mathematics *belle époque* 2
1.3 Lights and shadows. The von Neumann generation 7
1.4 Student years in Germany 13

2 Von Neumann and the Mathematics of Göttingen
2.1 The Göttingen mathematicians 19
2.2 Hilbert's mathematical optimism 22
2.3 The problems of the foundations of mathematics and the axiomatic approach 25
2.4 The axiomatization of quantum mechanics and functional spaces 31
2.5 A crucial contribution to game theory 39

3 A Mathematician Between Past and Future
3.1 Continuity and evolution in von Neumann's thought 49
3.2 Axiomatics and the twentieth century renewal of mathematical practice 53
3.3 Von Neumann's conception of mathematics 57
3.4 The language of mathematics and determinism 60
3.5 The world as a strategic game: a mathematical idea of rationality 67

4 Von Neumann in the United States
4.1 Princeton and the American mathematical community in the 1930s 75
4.2 A lucky migrant 79
4.3 Scientific commitment during World War II 82

4.4 From the Manhattan Project to the Atomic Energy Commission 87
4.5 Scientific research and national security during the Cold War 91
4.6 Freedom and limitations in the development of science and technology 96
4.7 Systems, information, control 104
4.8 Von Neumann's final years: a very engaged expert and the time stealthily borrowed for the scientist's projects 115

5 Beyond Mathematics: von Neumann's Scientific Activity in the 1940s and 1950s
5.1 From rational economy to the axiomatization of economic behaviour 121
5.2 The theory of games: a new mathematics for the social sciences 128
5.3 Decisions, organization, operations research 133
5.4 Engineering and mathematics: the project of an electronic calculator 138
5.5 The use of computer in scientific research 147
5.6 The brain-computer analogy 154

Concluding Remarks: von Neumann and Twentieth Century Science 167

Chronology 173

Bibliography
Sources and scholarship on John von Neumann 175
John von Neumann's published works and archived papers 176
Works by von Neumann mentioned in the book 177
General bibliography 181

Index of Names 201

Abbreviations

AF	Air Force
AEC	Atomic Energy Commission
AMP	Applied Mathematics Panel
AMS	American Mathematical Society
AOD	Army Ordnance Department
BRL	AOD Ballistic Research Laboratory
CalTech	California Institute of Technology
CIA	Central Intelligence Agency
ECP	Electronic Computer Project
EDVAC	Electronic Discrete Variable Computer
ENIAC	Electronic Numerator, Integrator, Analyser and Computer
FAS	Federation of American Scientists
IAS	Institute of Advanced Study, Princeton
IBM	International Business Machines
ICBM	Intercontinental Ballistic Missile
JNCW	*John von Neumann: Collected works*, edited by A. Taub, New York, Macmillan, 6 volumes, 1961–63.
JNLC	John von Neumann Papers, Manuscript Division, Library of Congress, Washington D.C.
MAA	Mathematical Association of America
MIT	Massachusetts Institute of Technology
NBO	Navy Bureau of Ordnance
NDRC	National Defense Research Committee
NRC	National Research Council
ONR	Office of Naval Research
OSRD	Office of Scientific Research and Development
RAND	Research and Development Corporation
RCA	Radio Corporation of America
SAGE	Semiautomatic Ground Environment
SIAM	Society for Industrial and Applied Mathematics
UN	United Nations Organization

Introduction

Matters at court devour my time & energies, as always. His Majesty becomes daily more capricious. At times he will forget my name, and look at me with that frown, which all who know him so well, as if he does not recognize me at all; then suddenly will come an urgent summons, and I must scamper up to the palace with my star charts & astrological tables. For he puts much innocent faith in this starry scrying, which, as you know well, I consider a dingy business. He demands written reports upon various matters, such as for instance the nativity of the Emperor Augustus and of Mohammed, and the fate which is to be expected for the Turkish Empire, and, of course, that which so exercises everyone at court these days, the Hungarian question: his brother Matthias grows ever more brazen in his pursuit of power.

– John Banville, *Kepler*.

In the lands situated in the heart of Europe, swept by the conflicts of the early twentieth century – just as they were three centuries before, at the time of Kepler – began the life of John von Neumann, a figure who perhaps more than any other is representative of twentieth century science. Von Neumann was a brilliant scientist, the author of fundamental contributions to mathematics and theoretical physics, and he played a vital role in the extraordinary display of power of twentieth-century science, as expressed by the development of computers, automation, space travel, and the use of atomic energy.

In fact, the triumphs of science – and also its miseries – which so deeply mark our era, are actually the outcome of a long historical process that begins symbolically with the work of Galileo. After the Scientific Revolution, the role of natural philosophy in modern society thinking and life became increasingly important. Even when this role was expressed in essentially theoretical and intellectual forms – that is, without any visible practical fall-out on everyday life – scientific breakthroughs, and above all the best-known of them all, Newton's mechanics and his theory of universal gravitation, formed the most solid basis for faith in progress, typical of the Enlightenment period and later of the whole of modern thought.[1]

1) On the principle of omnipotence characterizing modern scientific thought, see Israel 2001.

The links between von Neumann's own epoch and the distant age of the birth of modern science are closer than they might seem. In the first place – despite the emergence of relativistic and even sceptical views –, because science has continued to draw inspiration from the ideal of achieving a unitary, coherent, objective and universal image of the world. And secondly because, except for a few important differences, Kepler's description of the servitude and difficulties of his life as a "scientist" also evokes the delicate role that befell the so-called "experts" or scientific advisors after World War II. Kepler was appointed imperial mathematician to the court of Kaiser Rudolph II in 1601: in view of his knowledge of astrology, his main task was to advise the emperor on the management of a wide range of personal and political matters, and to deliver his prophecies in the form of "written reports". Von Neumann, although born in Budapest, in Hungary – that European frontier land between East and West on which the concerns of Rudolph II were focused – rose to the highest rank possible for a scientist at the service of the United States government. The dramatic upheavals that began to shake Europe in the 1930s led him, like many other scientists, to move to the United States, where he became a member of the select and exclusive Atomic Energy Commission. And it may well be said that several of von Neumann's writings prompted by his government service could well be considered prophecies concerning the times in which we live.

But here the analogies cease and differences arise. Indeed, the growing role of science in society and the increasing interweaving of science and technical knowledge which, towards the end of the nineteenth century, culminated in the burgeoning of technology, allowed less scope for the figure of the scientific scholar serving as an advisor at the mercy of the "king's" whims. The Age of Enlightenment had put forward the idea that the government of society must be founded on scientific bases, and indeed it is the scientific *élite* itself who should lead society in accordance with these principles. The *Idéologues* school led by the mathematician Condorcet[2] dreamed of discovering and applying the mathematical laws that would rationally and justly regulate the making of decisions in courtrooms, in assemblies, and in elections, or else would be used to govern the economy.[3] This highly ambitious project came to nought: significatively enough, Napoleon opposed the rights of human subjectivity and history to the Enlightenment's claim to base the government of society on the principles of pure rationality.[4] Nevertheless, the seeds had been sown of a dialectics that is still

2) On these topics see Moravia 1974, 1986.

3) Condorcet was the author of a program aimed at the establishment of a "social mathematics", that is, a mathematics suitable for treating all the problems involved in managing society and the economy. In this connection, see Baker 1975, Israel 1993a, 1996b.

4) In a speech delivered to the Council of State on 20 December 1812, Napoleon Bonaparte, in the following terms, attacked the movement of the *Idéologues* and its claim to construct a science of society: «It is *ideology*, this shady metaphysics that, subtly seeking the prime causes, sets out to establish on these bases the legislation of peoples, instead of appropriating laws to the knowledge of the human heart and lessons of history, that must be blamed for all the misfortunes that have befallen our fair France. [. . .] When one is called upon to regenerate a state, it is necessary to follow constantly opposing principles. History paints the human heart: it is in history that one must seek the advantages and disadvantages of the different legislations.» (N. Bonaparte, *Correspondance*, 32

being discussed and is still unresolved. And it was on the basis of the highly developed prestige and role of science that a multi-faceted genius, with an extraordinary capacity for navigating not only the paths of science but also those of government, was given the chance to succeed, albeit in isolation, where the followers of Enlightenment had failed. Von Neumann was not only a respected government advisor but, acting in this capacity, he succeeded in communicating and even putting into practice his idea that the governance of worldly matters must be guided by a universal logic within which each individual must move in accordance with a rational strategy directed to achieve the best result, taking into account the fact that also the other individuals are pursuing the same aim.

The life and scientific activity of von Neumann may be divided into two main periods, the time before and the time after his move to the United States. These two periods correspond to widely differing scientific interests. The European period is characterized by fundamental contributions to the great scientific issues of the early twentieth century, and will be dealt with in the chapters 1 and 2 of the book. The American period instead reflects the more usual and consolidated image of this scientist: it is no coincidence that the Hungarian emigré scientist is generally referred to by the Anglicized version of his first name, John (or Johnny). In this period science attained a far-reaching influence, not only in philosophy and culture, but also in the social and economic fields, and even in politics. This was the period that saw the birth of "big science", that is, scientific practice based on large-scale research projects linked to technological development and carried out by very large groups of specialists working in different fields, in large research centres provided with highly complex equipment and infrastructures and receiving very substantial funding. Chapters 4 and 5 are devoted to the contributions made by von Neumann to this line of development. Chapter 3 aims at outlining his overall scientific outlook, which in our view was consolidated already in the early years after his move to the United States. We will then follow the unfolding and development of this scientific outlook in the widely differing fields in which he was active: digital computers, the theory of automata, meteorology, game theory, and many other aspects of mathematical modelling.

Von Neumann died suddenly in 1957, aged only 54, as a result of a bone cancer that rapidly laid him low. Several important projects, such as his theory of automata, conceived at the height of his extraordinary scientific career, thus did not get off the ground. And yet, to view his life from a vantage point that is now becoming increasingly remote from the end of the Cold War affords an overview of twentieth century science. This book sets out to illustrate this vast and complex historical panorama without overstepping the inevitable bounds imposed by the need for a measured and non-specialist treatment.[5]

---

voll., Paris, 1858–69, vol. 24, n. 19390: 398–399). It is instructive to compare this passage with the claim made by François Quesnay, the founder of the physiocratic movement and one of the fathers of the scientific conception of economics: «Let us not seek lessons in the history of nations or human dismay, which portrays to us only an abyss of disorder» (Quesnay 1767).

5) An overview of sources and scholarship regarding von Neumann is provided before the bibliographical references at the end of the book.

No special knowledge is required, though some extremely difficult scientific concepts are discussed. To give a simple and at the same time exhaustive treatment of these issues would have required a book three times as large. In a few places, we opted to avoid trying to explain everything exhaustively. In these cases, we have tried to convey an "impressionistic" idea of these concepts, so that readers can get a general drift of the arguments, and then to guide readers who wish to go deeper into these matters to specialist publications that will help them. It is also possible to skip some of these passages without missing anything very important. Indeed the main aim of the book is not technical but to acquaint as large a reading public as possible with the scientific and cultural significance of one of the greatest figures of twentieth century science and technology.

Chapter 1

János Neumann's Early Years

1.1 A Jewish family in early twentieth century Budapest

Our hero was born in Budapest on 28 December 1903. János Lájos Neumann was his name in Hungarian, Jancsi for short. He was the eldest son of Miksa and Margit Neumann, members of the large Jewish community resident in the capital. Hungary at the time was still part of the Austro-Hungarian Empire, within which it enjoyed a substantial political autonomy governed by rules established in 1867, when Franz Joseph I was crowned King of Hungary.

During the second half of the nineteenth century, Hungary enjoyed a period of relative peace and industrial development, and had increasingly taken on the appearance of a modern European country, even though its economy continued to be based essentially on agriculture. The aristocracy had retained its position as the ruling class and governed the country's destiny from the capital, although its foreign and military policy depended on Vienna, the centre of the Empire. The Hungarian establishment displayed great dynamism that did not appear to be hindered by the new social tensions caused by the contrast between the backwardness of the peasant world, by the formation of an industrial proletariat or by the problems entrained by the coexistence of numerous national minorities, mostly of Slav origin (Croats, Romanians, Slovaks, Serbs).

The Jewish minority was relatively large and played quite an important role.[6] At the beginning of the century, about half the population of Budapest was made up of Jewish families. Many of these, initially living off the land in the poorer rural areas, later moved to the capital and improved their social position as the country's economy developed. Jancsi's mother, Margit, came from a well-to-do family: grandfather Jakab Kann, although of humble origins, had made his fortune trading in farm implements.

6) In 1919 almost a million Jews formed 4,5% of Hungary's population. See McCagg 1972, 1997, Rozenblit 2001, Gerö 2007.

Miksa Neumann was a lawyer who, at the birth of his eldest son, had been employed for some time as the director of one of the largest Hungarian banks, the Magyar Jelzáloghitel Bank (Hungarian Mortgage Bank). In 1907 his second son, Mihály, was born and in 1911 the third, Miklós.

As well as being the political centre of Hungary, Budapest, where most of the aristocracy resided, was also its cultural centre and reflected both the liveliness and the contradictions of an age of rapid economic and social development. The ruling class of the time was particularly open and ready to absorb the more enterprising and capable. Nevertheless, the educated social sectors were riven by divisions and contradictions. In particular, society was split into the "official" sector, which held traditional political and social views, and a minority of sympathizers with a constantly growing socialist movement, which planned to carry out a profound reform of the country. The radical party, composed mainly of physicians, lawyers and businessmen, many of whom were of Jewish origin, championed reform and universal suffrage and moved closer and closer to the socialists.[7]

The position of the Neumann family, like that of other well-to-do Jewish families in the capital, was both extraordinarily promising and unstable. The professional career of a Budapest Jew was apparently limited only by his personal capability. And yet the difficult situation regarding the country's equilibrium, the growing political weakness of the Empire, and the ever-present risk of new waves of anti-Semitism, threatened to perturb this favourable situation at any moment. Miksa Neumann was determined to enter the establishment, without, however, renouncing his religion, although the Neumann family did not observe Jewish religious rules.[8] Like other Jewish bankers and industrialists in the capital, in 1913 he procured a noble title granted by the emperor Franz Joseph. The family surname thus became Margittai Neumann, where "Margittai" stood for "of Margitta"). In the German version of János's surname, the particle "von" was added as the translation of the final "i", in reference to the acquired noble title.

1.2 A young talent in Hungarian mathematics *belle époque*

János Neumann received an excellent education and took full advantage of the exciting cultural atmosphere of Budapest at the beginning of the twentieth century. He was

7) Péter Hának has described Vienna as "the garden" and Budapest as "the workshop" (Hának 1998). See Jászi 1929, Horvath 1966, de Ferdinandy 1967, and Gerö 1995.

8) Grandfather Jakab Kann was a strict observer of Jewish religious practices and cultivated Talmudic studies. Miksa's family lived in a non-religious fashion, even exchanging gifts at Christmas time, although they did not fail to observe Jewish feast days. According to János' younger brother, Miklós, Jewish festivities were celebrated in the family as "non-denominational family reunions". In the United States, Miklós Neumann adopted the name of Nicholas A. Vonneuman and his recollections of his brother are contained in Vonneuman 1987. Information from interviews with him and other people regarding von Neumann's youth are gathered in Heims 1980, Chapter 2.

taught by private tutors until the age of ten and then pursued secondary school studies at the Lutheran Gymnasium, one of the best teaching establishments in the capital. The school had excellent teachers and permitted differential religious instruction, so that János was able to study the Hebrew language and literature with a rabbi. At the same time, he was given specific private tuition in mathematics. A scholar of excellent reputation, László Rátz, who had played an active role in the organization of modern mathematics syllabuses in Hungarian schools (as a member of the 1906 Mathematics Reform Commission), was professor of mathematics at the Lutheran Gymnasium. Rátz informed the family of the extraordinary mathematical ability displayed by the young János right from the first few months of attendance at the Gymnasium and advised his father to ensure that the young student received supplementary tuition. He saw to it personally that a private tutor was found: to do so he consulted a professor of mathematics at Budapest Technical University, Jószef Kürschák, who recommended several teachers capable of developing János' enormous talent.

During the closing decades of the nineteenth century, mathematical studies in Hungary underwent spectacular growth at all levels of the educational system. Growing scope was allocated to mathematics in technical high school syllabuses. The University of Budapest, the Technical University (established in 1871), and the University of Kolozsvár (established in 1872), catered to the growing demand for secondary school teachers and engineers. University circles played an important role in modernizing the country's mathematical culture: a typical figure was Gyula König, professor at the Budapest Technical University, whose textbooks and institutional activities made a strong contribution to the development of Hungarian mathematical research. A national group of mathematicians benefiting from autonomous institutions was rapidly forming in those years.

In this way, Hungary became part of the process of renewal of the scientific institutions that had changed the face of European science during the second half of the nineteenth century. Previously, the academies had been the main driving force behind scientific activities. From now on, although the academies retained an important role, specific scientific disciplines began to acquire an autonomous organization, in particular mathematics. One first step in this direction was the founding of periodicals specifically dedicated to mathematics, beginning with pioneering ones like *Annales des mathématiques pures et appliqués*, founded by the French mathematician Joseph Diaz Gergonne in 1810, and *Journal für die reine und angewandte Mathematik*, founded by August Leopold Crelle in 1832. Later the first mathematical scientific societies were set up: in 1864 the Moscow Mathematical Society and in 1865 the London Mathematical Society. Towards the end of the century there was a tendency to set up national mathematical societies, each with its own review, such as the Société Mathématique de France (founded in 1873) and the Deutsche-Mathematiker Vereinigung (founded in 1890). These societies gathered together all those in the country with an interest in mathematics: university professors, secondary and technical school teachers, engineers, and the military.

At the turn of the century, this model was extended to other countries of less prominent mathematical tradition and played a key role in fostering the exchange

of scientific information and in the development of original research inside individual countries. In Hungary, König, with other colleagues of the Technical University, founded the journal *Müegyetemi Lapok*, which was short-lived owing to lack of funds and subscriptions. However, several other initiatives destined to endure were consolidated. In 1885 an association of mathematicians was set up in Budapest by König and other scholars, including Baron Loránd Eötvös, a distinguished physicist who was also Minister of Education in 1894 and 1895. In 1891 an offshoot of this society was the Hungarian Mathematical and Physics Society – later known as the János Bolyai Mathematical Society – which initially counted nearly three hundred members. A few months before the constitution of the society the review *Mathematikai és Physikai Lapok* (Journal of mathematics and physics) began publication. However, the decision to use the Hungarian language was a serious obstacle to communication with mathematicians in other countries. The Academy of Sciences helped overcome this obstacle in 1883 by founding the review *Mathematische und Naturwissenschaftliche Berichte aus Ungarn*, which contributed to disseminating the work of Hungarian mathematicians; on the other hand, a large number of the latter had begun publishing articles in foreign reviews.[9]

Two initiatives represented a valuable contribution to the process of training young Hungarian mathematicians: the foundation in 1894 of the review *Középiskolai Matematikai Lapok* (Mathematical journal for the secondary school) by Dániel Arany, who was succeeded three years later by Rátz, János Neumann's mathematics teacher; and the organization, the same year, by the Mathematics and Physics Society, of a "mathematics competition among pupils" for the Eötvös prize, in honour of the Minister of Education. The above-mentioned review proposed exercises and problems in elementary mathematics and had the same structure as other European reviews circulating widely among teachers and students in their final school years, such as *Nouvelles Annales des Mathématiques*, published in France for candidates sitting for the awesome entrance examinations to the Paris *Grandes écoles*, such as the École Polytechnique and the École Normale.

Symbolic of the development of mathematics in the country at the turn of the century was the institution of the Bolyai prize by the Academy of Sciences – in honour of the famous Hungarian mathematician, János Bolyai, one of the inventors of non-Euclidean geometry – first awarded to the great French mathematician Henri Poincaré. Those years marked the beginning of the career of two mathematicians, Lipót Fejér and Frigyes Riesz, whose ideas were to lay the cornerstone of Hungarian mathematics research in the first half of the century. Both were Jews (although Riesz later converted to Calvinism), which seriously affected their careers. In Hungary at that time, as in Germany and Italy, there were already a large number of Jewish mathematicians. Science had never been a main concern in Jewish cultural tradition, although as a result of the emancipation and gradual conquest of civil rights in the various European countries, a growing number of Jewish intellectuals had access to

9) An overview of the organization of the national mathematical communities can be found in Grattan-Guinness (ed.) 1994; on Hungary see Szénassy 1992 and Hersh, John-Steiner 1993.

academic careers and thus developed an interest in scientific disciplines. However, it was very difficult for a Central European Jewish intellectual to obtain a university chair: the choice of mathematics was partly due, as David Rowe pointed out, to the autonomous and ideologically "neutral" nature of mathematics in the realm of knowledge (Rowe 1986). The ideal profession for those who had not yet obtained full equality of rights and were potential migrants had to be universalistic, international, and apolitical: and nothing approached this model more closely than the scientific disciplines, in particular mathematics (Israel, Nastasi 1998, Israel 2004d).

An anecdote related by Reuben Hersh and Vera John-Steiner clearly illustrates the rapidity with which Hungarian mathematics developed in relation to the broader cultural and social context (Hersh, John-Steiner 1993). In 1905, Poincaré went to Budapest to receive the Bolyai Prize and was given red carpet treatment, perhaps above all because he was a cousin of Raymond Poincaré, a French politician who later became President of the Republic and was three times Prime Minister under the Third Republic. Many ministers and VIPs had come to greet him but Poincaré, as soon as he got off the train, asked: "Where is Fejér?". The welcoming committee were surprised and asked: "Who is Fejér?" and Poincaré promptly retorted: "Fejér is the most important Hungarian mathematician, one of the most important mathematicians in the world". One year later, Fejér – who had not yet been given any teaching post – was appointed Professor at the University of Kolozsvár, and in 1911 was awarded a chair at the University of Budapest. Also Riesz, who had studied at Zurich, Göttingen, and Paris, was given a chair at the University of Kolozsvár, which, after Transylvania was transferred to Romania in 1920, was moved to the city of Szeged.

In the early decades of the century, the main core of Hungarian mathematicians was divided into two groups: Fejér's group at the University of Budapest, to which Marcel Riesz, Frigyes' brother, György Pólya, Ottó Szász, Mihály Fekete and Gábor Szegö belonged, and that of the University of Szeged, composed of Riesz, Alfréd Haar, Béla Kerékjártó and László Kalmár. Fejér's main interest lay in classic mathematical analysis. His most significant contribution was contained in his Ph.D. thesis and had been summarized in the review "Comptes rendus de l'Académie des Sciences de Paris", and in an extended version in *Mathematikai és Physikai Lapok* (Fejér 1900, 1902). It consisted of a new generalized method for summing Fourier series. Fourier series (introduced in the early nineteenth century by the French mathematician Joseph Fourier) are infinite sums of combinations of trigonometric functions, and one fundamental theorem of the theory affords the possibility of expressing each function satisfying fairly elementary hypotheses as the sum of such a series. This makes it possible to conceive of many phenomena as the superposition of a number of elementary periodic phenomena. The Fourier series found their first important application in the theory of heat and soon became a central topic in nineteenth century analysis and mathematical physics. Nevertheless the development of Fourier analysis was hindered by the difficulty involved in establishing rigorous conditions of convergence (that is, the conditions in which the series has a "sum"). Fejér's work addressed this particular aspect and led to a revival of Fourier analysis. Even though Fejér continued later to publish valuable contributions,[10] his scientific career was strongly hindered by the

vicissitudes of Hungarian politics in the period between the two world wars, and in particular by racial persecution of Jews.

Frigyes Riesz had a more straightforward career. For various reasons he managed to avoid the difficulties of those years, even though he only obtained the Budapest university chair after the war, because Fejér was already professor in the capital and there was a law against having another Jewish professor in the country's main university. Together with Haar, Riesz set up a research institute at Szeged, the Bolyai Institute, and also founded the review *Acta Scientiarum Mathematicarum*. He wrote numerous articles on the new approach to the problems of mathematical analysis, which had begun to emerge at the end of the century and made use of tools and analogies drawn from other branches of mathematics, in particular algebra and topology. The forerunner of this new research approach in the late nineteenth century was the Italian mathematician Vito Volterra, the author of fundamental contributions to the calculus of variations and the theory of differential and integral equations. However, Volterra did not give priority to the abstract approach in his research on what he had called "functions of lines" theory; his scientific outlook was still that of classical analysis, linked to geometry and physics. In the early twentieth century, the abstract approach received a huge impulse from the work of David Hilbert at Göttingen and Maurice Fréchet in Paris. The new methods, which covered a wide range of topics, opened up the way to a new branch of mathematics known as "functional analysis" of which more will be said later. The group of mathematicians who elaborated on Hilbert's research included Riesz, and the German Erhard Schmidt and Ernst Fischer. The former provided a demonstration for an important theorem, now known as the Riesz-Fischer theorem, and carried on work on the linear spaces of functions and the theory of operators defined on such spaces.[11]

Hungarian mathematics was thus quite open to the new trends emerging in early twentieth century mathematics. In addition to classic and functional analysis studies, another branch of research undergoing significant development in this country was what is usually called today "discrete" mathematics (in opposition to the mathematics of the continuum). This branch consists of a complex of mathematical techniques designed to address problems of a "finite" nature, such as classical combinatorial problems: for example, all possible configurations of a certain type that may be found in a finite (or discrete) set are studied and enumerated, together with the operations that may be defined on such a set, the correspondences between finite sets, and so on. Research in this field has a longstanding tradition: it was, however, overshadowed from the seventeenth century on by the supremacy of Newton's and Leibniz' infinitesimal

10) See, for instance, Fejér 1904.

11) This was a typical new chapter of functional analysis, which emerged as a development of Hilbert's theory of integral equations (Bernkopf, M. 1966–67). Moreover, also Volterra's contribution was closely linked to the theory of integral equations (Israel 1984, 1991). Schmidt's work includes extending the concept of eigen-function to integral equations with non-symmetric nuclei (Schmidt 1907). In the same year Riesz obtained (Riesz 1907) the well-known theorem today called Riesz-Fischer's theorem. It was actually re-demonstrated the same year by Fischer using the notion of median convergence (Fischer 1907a, Fischer 1907b). In a subsequent fundamental work Riesz introduced the concepts of weak convergence and strong convergence (Riesz 1910).

calculus, which placed the mathematics of the continuum at the focus of research, namely problems having an infinite nature and in particular those in which infinity has the property of being "continuous" (which meant, in the rough and ready insights characterizing the early phases of the research, without "interruptions" or "gaps"). At the beginning of the twentieth century, in relation to the growing influence of the algebraic approach and the development of set theory, discrete mathematics underwent a renewal in which an important role was played by Hungarian mathematics. Following the path opened by Gyula König's seminal work on set theory,[12] the pioneers in this field were König's son, Dénes – the author of a fundamental book on the theory of finite and infinite graphs, published in German in 1936 (König 1936) –, László Kalmár and György Pólya.

This brilliant prospect for Hungarian mathematics was clouded by the defeat of Austro-Hungary in the Great War, which had a negative effect on the flourishing world of Hungarian mathematics, as well as on every other aspect of the country's life. The Bolyai prize, received by Hilbert in 1910, was no longer awarded owing to the devaluation of the fund supporting it. Many young mathematicians were left jobless and were obliged to give private lessons to make ends meet. This was the case of two young mathematicians in Fejér's group, who were private tutors to János Neumann, first Gäbor Szegö then, after the latter left Budapest, Mihály Fekete.

This was how von Neumann came into contact with the brilliant mathematical tradition of his own country which had such a strong influence on the direction taken by his research. His studies together with Fekete were fruitful: even before he had completed high school, the two wrote an article in collaboration, in which they generalized one of Fejér's results on the localization of the roots of Chebyshev's polynomials, published in 1922 in the journal of the Union of German Mathematicians *Jahresbericht der Deutschen Mathematiker-Vereinigung* (Fekete, Neumann 1922). Jancsi continued his secondary studies, gaining full marks and, in the end, was awarded the Eötvös national prize like his illustrious predecessors: Fejér, Haar, Marcel Riesz, his tutor Szegö, Kalmár, and the physicist Tódor Kármán (who changed his name to Theodore von Kármán after migrating to the United States) and Ede Teller (Edward Teller).

1.3 Lights and shadows. The von Neumann generation

The formative years of the young Jancsi were extremely encouraging. His lively mind, which had already set him apart from his peers, was stimulated by brilliant teachers who introduced him to a wide range of philosophic and scientific topics. Numerous anecdotes are told about the quality of this true "enfant prodige", his astonishing memory, his insatiable thirst for knowledge, his critical mind and his penchant for discussion. One of the more vivid ones is told by the physicist Eugene P. Wigner (whose original name in Hungarian was Jenö Pál). In an interview he gave to Thomas

12) For an extensive bibliography on Gyula König's work in this and other fields see Szénássy 1992.

Kuhn, now a famous Nobel physics laureate, this is how he remembers his school companion Jancsi:

> *We often took walks and he told me about mathematics and about set theory and this and that. It was amazing. And he loved to talk about mathematics – he went on and on and I drank it in.*
>
> *He was inexhaustible on such occasions in telling me about set theory, number theory, and other mathematical subjects. It was really wonderful. He never thought of going home* [...] *He was phenomenal, also in his desire to talk.* [...]
>
> *Particularly from having known Jancsi von Neumann, I realized what the difference was between a first-rate mathematician and someone* [*like me*].[13]

However, even though the personal qualities of this young man were astonishing, there is no doubt that the world he was being formed in was also particularly stimulating and reflected the dynamic quality of a dizzily expanding, culturally rich and lively, society. In the preceding section we saw Hungarian mathematics as a vigorous reality. Therefore, although von Neumann's training was destined to be moulded mainly by input from the Göttingen school of mathematics and its leader David Hilbert, it would be an error not to take into account the influence the Hungarian environment had on him. The importance of functional analysis and set theory was perceived by von Neumann in Hungary even before it was in Germany, and his very marked attention to combinatorial analysis and finite and discrete mathematics were strongly rooted in the research of Kalmár and Dénes König.

We shall have occasion later to discuss in detail one of his greatest contributions to twentieth century science: the foundation of the modern mathematical theory of games. And yet in this case also it is clear that his early interest in this topic is linked to his Hungarian experience.[14] This link must be considered not only in a strictly technical sense but in the broader cultural and conceptual sense. As we shall see, for him game theory was to represent a very general form of mathematical analysis of the problems of conflict between individuals pursuing contrasting ends. In other words, it was no longer to refer only to ordinary parlour games, but to the much wider and more "serious" field of social and economic conflict. Von Neumann is unanimously considered one of the scientists of the last century who paid most attention to social and economic applications and topics, and though the birth of this interest is often traced back to his commitment to the field of military applications, which began during World War II, this is not enough to account for the special passion he displayed for this topic and which is not to be found in many other scientists, for whom military

13) Interview of Wigner by T.S. Kuhn, *Archives for the History of Quantum Physics*, American Philosophical Society Library, Philadelphia, Pa., tapes 92b and 93a. See Heims 1980, 42–43.

14) The cited book by Dénes König on graph theory (König 1936) contains two interesting references to von Neumann to which we shall return and which refer also to game theory.

and industrial commitments represented a parenthesis after which they returned to their pure research studies.

The interest in socioeconomic theory and technological and industrial problems openly displayed from the 1940s on was rooted in von Neumann's conviction that they could be subjected to exact scientific treatment – a conviction that can be traced back to the cultural climate of Budapest and in particular to the atmosphere of enlightened rationalism current in his own family under the influence of Miksa Neumann. His father's economic and social activities brought János into contact with more worldly matters, the successes and hazards of real life. Miksa Neumann's home was assiduously frequented by colleagues, businessmen, and well-known Budapest intellectuals. This is what his brother Nicholas A. Vonneumann – to use the American version of his name – remembers of those years:

> *Around the dinner table we listened frequently to my father's comments on his own profession and business activities, as a lawyer and banker engaged in financing, and his underlying theories, namely, financial statement analysis and criteria for credit qualifications; and the practical applications, namely, selecting investment opportunities or borrowers. This usually led to a general discussion of the underlying theories also of other professions in relation to their respective practical applications. Father also used to discuss the technological aspects which his industrial applications involved.*[15]

This portrays a vivid picture of the typical faith in the scientific and technical rationality of a social class undergoing and managing an impelling social and economical expansion process. However, it would be an error to define this attitude as a kind of positivistic optimism. As we will see, throughout his life, von Neumann's activities were based on a practically unbounded confidence in the power of reasoning, and of scientific and mathematical reasoning in particular. However, this confidence was accompanied by a lack of faith, or scepticism, and not just latent or implicit, regarding man's capacity to follow more reasonable and constructive paths. Faced with the irrational tendency that mankind often displays when it chooses the more disastrous and destructive alternatives, scientific discernment and rational determination guiding optimal behaviour seemed to be the only way of preventing the potentially detrimental effects of this human tendency, or at least of keeping it within realistic limits. Also this aspect of his mindset – of which obvious traces may be found in his scientific work on social science – has its roots in his Hungarian and European experience. The power of scientific and mathematical reasoning in the context of man's destructive behaviours were the light and shadows of his early life.

Indeed, Jancsi's childhood was not immune from serious upheavals which left their mark on his personality. The year he entered the Lutheran Gymnasium, World War I broke out and the subsequent defeat of the Austro-Hungarian empire brought a tremendous catastrophe for Hungary in its wake. This florid Central European state

15) Glimm, Impagliazzo, Singer (eds.) 1990, 20.

lost many of its territories and more than three million Hungarians were transferred under the sovereignty of three new states – Czechoslovakia, Romania and Yugoslavia. Charles IV, the grandson of Emperor Franz Joseph, who had succeeded to the throne on the former's death in 1916, abdicated in October 1918. Shortly before, Count Mihály Károlyi had been nominated prime minister of Hungary, but Károlyi failed in his attempt to found a democratic republic with the support of the liberal, radical and socialist forces and, in early 1919, power passed to the communists of Béla Kun. During the five months of the Hungarian Soviet republic the Neumann family sought refuge in Austria, between Vienna and Abbazia, on the Adriatic coast. This experience profoundly affected the political views of the young János and contributed to arousing in him a powerful hostility towards communism. It is significant in this regard that in 1955 he should acknowledge that

> *My opinions have been violently opposed to Marxism ever since I remember, and quite in particular since I had about a three months taste of it in Hungary in 1919.*[16]

Bankers, aristocrats, and conservative military in Vienna and Szeged, with the help of the countries of the Entente, prepared a counter-revolutionary movement that, after the invasion of Hungarian territory by the Romanian army, seized power under the leadership of Miklós Horthy. The situation in which the Neumann family found itself on their return to Budapest was anything but reassuring because of the powerful waves of repression and anti-semitism that followed the advent of the Horthy regime. The participation of many Jewish intellectuals – including Béla Kun himself – in revolutionary movements became a pretext for unleashing persecution. Horthy introduced a number of anti-semitic measures although, very pragmatically, he protected Jewish bankers and industrialists who were useful to him in the reconstruction of the country, in particular the ennobled élite. In this way, the Neumanns continued their everyday life without too many problems. In 1921 János passed the rigorous tests of loyalty to the regime and was admitted to a course of mathematical studies at the University of Budapest, despite severe restrictions placed on the number of Jews admitted to the University that had been set by parliament one year previously (5% of the total number of students).

Nevertheless, from that year on, János Neumann's life began to take a different course from that of his country. He first embarked upon an intense training phase in Germany, attending the University of Budapest only at exam time. In this and other aspects, the path followed by him was similar to that of many other Hungarian mathematicians and scientists of the time, most of whom were Jews. In an interview with Steve J. Heims, the economist William J. Fellner, born in Budapest and who later migrated to the United States, claimed that «many traits of von Neumann noted by Americans are just those of a good Budapester of his time and his social class; that he

16) This passage is contained in a hand-corrected draft referring to "Nomination of John von Neumann to be a member of the United States Atomic Energy Commission", 8 March 1955 (John von Neumann papers at the Library of Congress, JNLC, see Aspray 1990, 247).

was very much a Budapest type» (Heims 1980, 26–27). Von Neumann's personality was strongly affected by events in this early part of his life – both the political vicissitudes and the extraordinary cultural experience of that time. In particular, it is easy to understand the significant role played in this context by the struggle for success, the ineluctable need to stand out in the professional and social environment, and the inclination towards a committed public life – aspects characteristic of his personality that also distinguish it from the classical image of the scientist as a "wise man" shut up in his study or laboratory and remote from worldly matters.

Von Neumann himself described the salient features of his own experience and that of his generation in the following terms, as reported by his Polish mathematician friend Stanislaw M. Ulam: «An external pressure on the whole society of this part of Central Europe, a subconscious feeling of extreme insecurity in individuals, and the necessity of producing the unusual or facing extinction» (Ulam 1958, 1). This situation often led to a psychological state that could be described as the constant feeling that one was "living out of one's suitcase" and which encouraged an "internationalistic" attitude in perfect harmony with the most modern and advanced trends in early twentieth century scientific research. These were the characteristics and problems of an entire generation of high calibre Hungarian intellectuals who made an exceptional contribution to European culture. A generation that included not only the scientists we have already mentioned but also physicists like Eugene P. Wigner, Leo Szilard and Dénes Gábor, as well as the mathematician Abraham Wald, the economist Nicholas Kaldor (the Hungarian Finance Minister, who later became a naturalized Englishman), and the already cited Fellner, the sociologists Oszkar Jászi and Karl Mannheim, the philosopher György Lukács,and the writer Arthur Koestler. Some of these were politically active in Budapest, while others, like von Neumann, preferred to distance themselves from the events taking place in their country.

The 1920s witnessed the beginning of a gradual wave of migration from Hungary by many mature scientists, as well as many talented young ones desirous of completing their studies, all now aware that staying in Budapest would lead to a dead end. Their destinations were above all the centres of Germanic culture like Berlin, Göttingen, and Zurich. Indeed, for a country like Hungary – tormented by tension with the Slavic east represented by the less well educated classes and by the constant threat from Russia – the German area culture represented a chance for the country's inclusion in the modern and advanced currents of western Europe. Subsequently, in the 1930s, many of these scientists migrated to the United States, first because of the difficulty of finding teaching posts in Germany, due to the inflated numbers of young researchers, and immediately afterwards because of the rise of Hitler's national socialism. For example, von Kármán moved to Göttingen, then to Aachen, and ultimately to CalTech (California Institute of Technology). Marcel Riesz obtained a post as professor at Lund (Sweden), where he set up an important school of mathematics. Pólya went to Zurich, before settling in Stanford, in the United States. Fekete migrated to Israel. Gábor moved to the UK. Many others among von Neumann's peers, such as Teller, Wigner, Szilard and Paul Halmos, settled like him in the United States, forming a group of "illustrious immigrants" – to use an expression coined by

Laura Fermi, the wife of the Italian physicist Enrico Fermi – who remained in touch with each other throughout their lives and to which social and human scientists also belonged.

Further on we shall examine von Neumann's activities in the framework of the US military programmes. Here we shall only mention in passing this much debated aspect of his work and of his politics, since it is closely related to events of his early life. His daughter Marina, in open quarrel with those who criticized her father's belligerence, asserted that:

> *One purportedly serious evaluation, for example, combined pop sociology and Marxist ideology to conclude that von Neumann was driven by a desire for power and a fascination with being close to the rich and powerful.* [...]
>
> *But I believe that his wide-ranging activities, including his worldly involvements as well as his strictly intellectual pursuits, were motivated by two profound convictions. The first was the overriding responsibility that each of us has to make full use of whatever intellectual capabilities we were endowed with. He had the scientist's passion for learning and discovery for its own sake and the genius's ego-driven concern for the significance and durability of his own contributions. And the second was the critical importance of an environment of political freedom for the pursuit of the first, and for the welfare of mankind in general.*[17]

Many promising young Hungarians had already left the country when the political situation took a decided turn for the worse. In 1932 Gyula Gömbös, a declared fascist and anti-semite, was nominated prime minister and from that time on life became increasingly precarious. When the Germans entered Hungary in 1944, a collaborationist regime was set up and the Hungarian Jews were deported en masse. Fejér was never to recover from the trauma of those years. The banner of Hungarian mathematics after the war was passed on to Riesz, who became professor at the University of Budapest, and to the few others who remained. The onset of World War II extinguished the carefree and bright life of Budapest, so full of new experiences and intellectual adventures, offering in exchange only persecution and death. The sad destiny of von Neumann's generation is aptly summed up by Koestler in his book of memoirs published in 1952, *Arrow in the Blue:*

> *At a conservative estimate, three out of every four people I knew before I was thirty were subsequently killed in Spain or hounded to death at Dachau, or gassed at Belsen, or deported to Russia; some jumped from windows in Vienna or Budapest; others were wrecked by the misery and aimlessness of permanent exile.* (Koestler 1952, 99)

17) Glimm, Impagliazzo, Singer (eds.) 1990, 1–2. Note in this connection that von Neumann, in the already cited 1955 reference (see note 16) asserted: «I was probably a good deal more militaristic than most».

1.4 Student years in Germany

The influence of German mathematics, which was enjoying an unprecedented explosion of creativity, on the scientific personality of von Neumann was added to that of Hungarian mathematics. The flourishing of German mathematics and science was part of the rich cultural life characterizing the period of the Weimar Republic. Between 1921 and 1923 von Neumann attended several courses at the University of Berlin, the most famous university in Germany at the time; he attended chemistry lectures by Fritz Haber, statistical mechanics by Albert Einstein and he made the acquaintance of the mathematician Erhard Schmidt. Between 1923 and 1925 he studied chemical engineering at the Zurich Polytechnic, at that time one of the most prestigious higher technical teaching institutions in Europe. However, he did so only to satisfy his father who was pushing him towards studies that offered practical outlets, though his main interest now lay in mathematical research. Yet he profited from his stay in Zurich by making contact with the distinguished German mathematician Hermann Weyl and with his fellow countryman Pólya.

The Hungarian scholars, although plunged into a German speaking cultural environment, nevertheless continued to maintain contacts among themselves. However, continuously widening horizons opened up to von Neumann's interests. He spent the years of his general and scientific education among Budapest, Vienna, and Berlin. The numerous discussion centres that grew up in that stimulating period encouraged him to engage in wide-ranging intellectual pursuits. A large part of his involvement in not strictly mathematical topics dates back to this period: interest in technology and engineering (stimulated by his chemical engineering studies), in economic theory (as attested by Kaldor), in parlor games. As E. Roy Weintraub suggests (1985, 74), applied mathematics was the main topic of discussion in an informal club organized by his fellow countryman Szilard in Berlin, which was attended also by Gábor and Wigner.

Another extremely stimulating atmosphere was Karl Menger's Mathematisches Kolloquium in Vienna – attended also by Kurt Gödel – whose activities were followed by von Neumann and whose sessions he participated in at least once in the 1930s, with a seminar on his mathematical economics research. A fundamental point of reference at this time was the Vienna Circle, which grew up in 1925 around the seminar of the physicist Moritz Schlick, who held the chair of Philosophy of Inductive Science that had previously been held by Ernst Mach and Ludwig Boltzmann. The Circle, in which Otto Neurath, Hans Hahn and Rudolf Carnap – a disciple of the logician Gottlob Frege – participated actively, met periodically to discuss the philosophical consequences of aspects of the history of science and logic, especially the work of Mach, Poincaré and Pierre Duhem and later of Ludwig Wittgenstein's *Tractatus logico-philosophicus*, which more than any other work served as inspiration for the group. Von Neumann participated directly in the discussions promoted by the Vienna Circle on the foundations of mathematics.

The issue of the so-called "crisis in the foundations" in which mathematics found itself at the time after the discovery of logical paradoxes in Georg Cantor's

set theory, represented one of von Neumann's main intellectual concerns in those years. These problems had already been introduced into the Hungarian environment by Gyula König who, in the last years of his life, dedicated a book to "the new foundations of logic, arithmetic and set theory", published posthumously (König 1914). Von Neumann's first work on these topics related to the introduction of Cantor's transfinite numbers and was published in the first volume of Szeged's journal *Acta Scientiarum Mathematicarum* (Neumann 1923). During the same period, Schmidt sent a lengthy manuscript written by von Neumann entitled *Eine Axiomatisierung der Mengenlehre* (The axiomatization of set theory) to Abraham A. Fraenkel, a professor at Marburg University who had made a relevant contribution to these topics by perfecting, in a series of contributions published from 1921 on, the results obtained by Ernst Zermelo in a 1908 work.[18] Schmidt asked Fraenkel for an opinion on von Neumann's article which he found "incomprehensible". Fraenkel invited von Neumann to pay him a visit to discuss the work and to prepare another version of it in which the problem and the proposed new approach were couched in less technical and more informal language. The paper was published in 1925 in the *Journal für Mathematik*, of which Fraenkel was co-editor (Neumann (von) 1925). The expanded version of this research formed the substance of von Neumann's Ph. D. thesis entitled *Az altalanos nalmazelmelet axiomatikus foleptises*, which was presented in September 1925 at the University of Budapest, under the supervision of Fejér, and then published three years later in German in the journal *Mathematische Zeitschrift* (Neumann (von) 1928a).

Again in 1925, he had not completely abandoned his work in classic analysis: one of the few papers published by von Neumann in Hungarian (in the journal *Mathematikai és Physikai Lapok*) dealt with uniformly dense number sequences (Neumann 1925b). But his main concerns were now diverging from those of Fejér's school; in addition to his work on the foundations of mathematics, in Berlin Schmidt had introduced him to modern research into linear functional analysis both à la Hilbert and following Riesz's line of research, to which Weyl had made major contributions. We have already said that the radical novelty of this approach consisted in the use of abstract algebraic and topological techniques. Moreover, von Neumann's interest in algebraic questions is demonstrated in a paper he published in Szeged's journal concerning Heinz Prüfer's theory of ideal numbers (Neumann 1926). The idea behind this paper, as he acknowledges in the text (which was something exceptional, as this type of reference is found elsewhere only in several of his youthful publications), had been suggested to him by József Kürschák, professor of mathematics at the Budapest Technical University.

In the summer and autumn of 1925, on Schmidt's advice, he visited Göttingen for the first time, where he met the great Hilbert. Hilbert was a perfect interlocutor for discussions about mathematical logic, axiomatics and set theory and, in the years that followed, his teachings inspired von Neumann in his main lines of research. At Göttingen, a meeting point for scientists from all over the world, the young Hungarian scholar encountered several of the most famous mathematicians and physicists of the

18) Zermelo 1908; see also Chapter 2.

time, including the Americans Norbert Wiener and Robert Oppenheimer. During the academic year 1926–27, with the support of Hilbert and Richard Courant, he was awarded a scholarship by the Rockefeller Foundation to study the foundations of mathematics at Göttingen, and in 1927 he was created *Privatdozent* at the University of Berlin. Although he lectured at Berlin and in 1929 at Hamburg, the main focus of his research was still at Göttingen.

The first steps in this career were certainly made possible by the economic support received from his family. Prospects were not rosy for a young man yearning for an academic career in Germany: after earning a Ph.D. and a period of study spent at other universities, it was necessary to present a memoir, the *Habilitationsschrift*, in order to obtain the *Venia legendi*, that is to be able to hold courses as a *Privatdozent*. However, the *Privatdozenten* were not paid and had to support themselves out of the small fees paid by the students. Many distinguished German scholars were stuck in this position for a long time before succeeding in making the much desired leap to the status of Lecturer, and above all Professor, which represented one of the highest ranks in the country's social ladder.

At the end of the 1920s, in Germany, competition for professorships was fierce. It was certainly also for this reason that, at the end of 1929, he accepted the post of guest professor in mathematical physics at Princeton University offered to him by Oswald Veblen, one of the more distinguished members of the US mathematical community. Von Neumann was now 26 years old; his father had died recently and responsibility for the family had fallen upon his shoulders. Before leaving for Princeton he married Mariette Kövesi, a Hungarian Catholic, the daughter of a Budapest physician, who was a friend of the family. On this occasion von Neumann converted to Catholicism.[19] This marriage lasted seven years, ending in divorce in 1937. His only daughter, Marina, was born in the United States in 1935.

Between 1930 and 1933 he divided his time between the University of Berlin and his new American post. In January 1930 he was offered tenure at Princeton University, but he did not accept it and became instead Jones professor of mathematical physics, on a one semester per year basis for five years.[20] However, the situation in Germany was deteriorating rapidly: in January 1933 Adolf Hitler was nominated Chancellor of the Reich and, in the months that followed, there was a dramatic increase in antisemitic demonstrations. At the end of March that year, Einstein handed in his resignation to the Prussian Academy of Sciences and in April there was a wave of dismissals and resignations of university professors, which rapidly put an end to the brilliant Göttingen experience. In January, Einstein and Weyl accepted the invitation to take up a post of professor at the recently established Institute of Advanced Study at Princeton; after Weyl's retirement, the position was taken by von Neumann, who enjoyed Veblen's support and was now a very well-known mathematician.

19) According to the account of his brother Nicholas, in this circumstance the whole family converted to Catholicism «for the sake of convenience, not conviction» (Vonneuman 1987).

20) Letter to Rudolf Ortvay, April 17, 1934, in Rédei (ed.) 2005, 193.

In the tormented years leading up to World War II, von Neumann continued to travel to Europe every summer, visiting Italy, France, England, and Hungary, where his family still lived, before finally deciding to join him permanently in the United States. In his letters to Veblen written from Budapest in the years 1933–1935 (with the signature John) and to Rudolf Ortvay from Princeton in 1938–39 (with the signature Jancsi) he expressed a deep involvement in world affairs and especially in the future of Hungary. As to his native country, he considered that the imminent war would be a catastrophe for all state-forming nations of Europe; and wrote: «Really, I can only hope that southeastern Europe will be left out of this, partly because the possibilities there are very confused, partly because one always hopes for "miracles"».[21] At the same time, a mix of excitement and admiration for the United States emerged, as far as he considered that a war was inevitable and he tried to concentrate on the future of European culture. In fact, he established a parallel between the fate of the industrial-scientific modern civilisation and that of the ancient Greek civilization, as he wrote to Ortvay:

> *I can only partially accept your views on what Europe's decline would mean for the World.*
>
> *I too think that it would mean a gigantic cultural minus (actually we may speak of this already in the present indicative tense), and the U.S.A: could not completely substitute for it.*
>
> *But I think that the Greco-Roman analogy of the ancients is valid in this regard. Of course by Rome's "transplantation" of Greek culture much fine detail got lost, indeed important elements too. Yet this way the ancient civilization still could continue to exist essentially intact for at least 300 more years, and in many modified forms even far beyond.*
>
> *Judging the European Hellenes it must not be forgotten also that most of them are Thracians, Macedonians and Persians... Hence by strict Greek standards barbarians.*[22]

In the context of the European and German catastrophe, von Neumann broke with German culture and German mathematics, as it is clear from his letter of resignation from his membership from the Deutsche Mathematiker-Vereiningung addressed to its president Wilhelm Blaschke on January 25, 1935:

> *Although not a German, I am very much indebted to German science and especially to many representatives of mathematics and physics in every part of Germany. I had received my scientific education in the German speaking part of the World and have spent part of my scientific career in German universities – a part, which remains for me unforgettable for*

21) Letter to Rudolf Ortvay, February 26, 1939, in Rédei (ed.) 2005, 200.

22) Letter to Rudolf Ortvay, February 26, 1939, in Rédei (ed.) 2005, 200.

> *ever. I take special pride in having been active also in the University of Hamburg.*
>
> *Nevertheless I cannot reconcile it with may conscience to remain a member of the German Mathematical Society any longer, after another international member, Mr. H. Bohr, was condemned by the 1934 Assembly for having made a political statement abroad, the resolution was made public, and Mr. H. Bohr has drawn the consequences on this part.*
>
> *Therefore I must ask you to have my name deleted from the membership list. It is my hope that my paths and those of the D.M.V., whose true interests I still believe to be serving, are not separating for ever.*[23]

After the war he cultivated his European contacts and kept up correspondence in Hungarian with several distinguished researchers in his native country; from the 1940s on his scientific activity was concentrated on the United States. An anecdote recalled by von Neumann's daughter Marina illustrates well the tight ties that his father conserved to his European training and career. In early 1956 Marina brought her fiancé Robert Whitman home to Princeton to meet her father. Von Neumann wanted to show his future son-in-law the computer that was one of his main scientific projects after the war; outside the door of the Electronic building project von Neumann tried to find the key: «He went through all his keys. He said, "Here's my house keys, here's the key to the Swiss Institute of Technology from 1929, here's this one and that one"».[24] The bunch of keys in his pocket was a symbolic link between his American life and enterprises and the unforgettable years of his European training and career, which proved so important in determining his scientific trajectory.

This was a period of constant coming and going between Budapest, Göttingen, Berlin, Hamburg and Princeton, although the key to understanding von Neumann's research in this particular phase is Göttingen and the genius behind the group of mathematicians working at this small university – David Hilbert.

23) Letter to Wilhelm Blaschke, January 28, 1935, in Rédei (ed.) 2005, 70.

24) Interview by Mike Brewster, *Business Week*, April 8, 2004 ("The great innovator: John von Neumann: MANIAC's father").

Chapter 2

Von Neumann and the Mathematics of Göttingen

2.1 The Göttingen mathematicians

When von Neumann visited Göttingen for the first time, in this small university there was a remarkable, lively nucleus of mathematical research, whose ambitious cultural vision had spread well beyond that of the traditional centre of German mathematics, the University of Berlin. The organization of this nucleus was the result of the personal initiative of the great mathematician Felix Klein, who had obtained the chair at Göttingen in 1885. Göttingen's mathematical tradition retained at length the orientation given by Klein, although, starting in 1895, it was increasingly subjected to the influence of the different approach introduced by David Hilbert.[25] Klein had displayed a deep interest in physics and in the applications of mathematics from the outset, and had collaborated with the mathematicians Julius Plücker and Alfred Clebsch, who, during the second half of the nineteenth century, had represented the opposition to the "purist" tendencies of the Berlin school of mathematics. The "purist" geometry schools – which had sprung up during the century in several European countries, in Germany, France and Italy in particular, – were opposed to studying geometry by the analytical method of Cartesian origin, that is, based on the systematic use of algebra and analysis to represent and study geometric properties. In their view, the theorems, starting from the properties of the geometric entities under investigation, had to be obtained through "synthetic" arguments that were "purely" internal to the geometric discourse, that is, without the help of algebraic calculus. The conflict between the "purist" tendency and the "analytical" approach became very bitter. One example is the case of Plücker, who had become well known for his analytical study of projective geometry using "homogeneous" coordinates, a generalization of the Cartesian coor-

25) On these topics see Rowe 1989, Rowe 1986.

dinates designed to study projective space but gave up geometric research in favour of physics after heated discussions with the Berlin mathematician Jakob Steiner, who championed the cause of the synthetic study of geometry which he considered as the only admissible research approach in the geometric field. This he did in such an exclusive and radical way as to convince Plücker to make a change of tack. Georg Cantor – as a result of the controversy with Leopold Kronecker, one of the greatest Berlin mathematicians – was another illustrious victim of the arrogance and ideological preconceptions of the Berlin school of mathematics, and paid the consequences with his academic career. Indeed "purism", at the outset, represented a very fertile period for mathematics research, in which «all you had to do was to open your hand to gather an abundant harvest of discoveries»,[26] although it ultimately had negative effects as, by tending to provide a specific and isolated definition of the methods it considered characteristic of each single branch of mathematics, it ended up by isolating them from one another and hindering the fertile interactions that might have taken place among them.

After the deaths of Plücker and Clebsch, Klein became the point of reference for a large number of German mathematicians who did not intend to adhere to any exclusivistic research approaches, leaving a trace of his influence in all the centres in which he taught. Towards the mid-1890s, Klein's ideas crystallized into a project to set up at the University of Göttingen a group of mathematicians capable of reviving the tradition of this centre, at which eminent mathematicians like Gauss and Riemann had taught. Indeed, starting in the 1880s, Klein – after an exhausting "competition" with Poincaré on the topic of automorphous functions – gave up front line research and dedicated himself to the role of research organizer. The main underlying idea was identified as the centrality of the relationship between mathematics and physics and of the geometric approach for the study of all kinds of problems. Klein's main intention was to allow interaction among the various approaches and above all to involve high calibre researchers regardless of their topic and their method of research. One comparatively important aspect of his project was the development of an aggressive policy of increasing the number of mathematics chairs. In this he was aided by Friedrich Althoff, an official who exerted control over higher education in Prussia between 1882 and 1907.

Klein cleverly and pragmatically sidestepped the opposition to the presence of Jewish mathematicians in German university departments. Racial prejudice in Germany had taken a special form in the field of mathematics: the idea was spread about of the existence of a "Jewish style" characterized by a tendency towards abstraction, which was in contrast with the more intuitive and algorithmic German-Aryan style. Klein himself held somewhat contradictory views in this regard. On the one hand, he was anguished by the conflict that, in his opinion, was emerging between

26) Enriques 1920, 3. This article by the eminent Italian geometrician Federigo Enriques contains a brilliant description of the merits and excesses of the "purist" point of view. He was to refer to the witty remark made by a colleague according to which, in this first phase of impetuous development, geometry had become so popular that «it was enough to sow a bean seed to see a geometrician sprout» (*Ibidem*).

the Germanic mindset – concrete and naturalistic – and the rise of a new way of doing science that he believed to be inspired by forms of abstraction that he considered typical of the Jewish mentality. He loathed everything in science that smacked of "Jewish", "French" and "axiomatic" and considered the Jewish presence in science as a phenomenon of "national infiltration". He promoted seminars, which were debatable to say the least, in which specific racial characteristics of the way Jews reasoned in mathematics were theorized.[27] On the other hand, he accompanied these views with some peculiar political ideas according to which German mathematics had been going through a stage of stagnation, and the entry of the Jewish component had brought "new mathematical talents" that "had born fruit". This process, which he called "bringing fresh blood" *(Bluterneuerung)*, would allow the introduction of new abstract ideas to which the Jews would be particularly sensitive and which would further reinvigorate German mathematics. Nazi propaganda and the mathematicians of Hitler's regime later gave an extreme and one-sided interpretation of Klein's ideas; they singled out the racial aspects that were certainly contained in them, ignoring the fact that Klein had in any case turned Göttingen into a place where Jewish scientists could gather and where an open and tolerant attitude was maintained. Klein's ideas were converted into the paradigm of "Aryan" mathematics, thus exploiting his highly prestigious name to foster their racist propaganda.

This dynamism of Göttingen mathematics was heightened by Klein's view that mathematics should be conceived of within a broad, unified scientific context, on a par with the natural sciences and also technology. The relationships with the engineering and the industrial worlds were extremely concrete, and the relations that Klein himself maintained with German industry stimulated industrialists' growing support of scientific infrastructures and represented a remarkable example of private support for technological and scientific research. For these reasons, Klein began to occupy an important position in the German political and economic scene. At the university he promoted contacts among mathematicians and physicists and engineering specialists in fields such as hydrodynamics, technical mechanics and the theory of elasticity.

One of Klein's first and most brilliant "recruits" was Hilbert, who shared with him a belief in the importance of a broad conception of mathematics and of the power of unifying ideas as opposed to specialization and "purism". Hilbert was also convinced of the fact that one fundamental condition for the development of mathematics research was to abandon the enclosure of the national schools and to adopt an attitude of strong international spirit. Under the leadership of Klein and Hilbert, the Göttingen school of mathematics became an important phenomenon typical of the culture of Weimar.[28] In the period between the two world wars, Hilbert's influence

27) In one of these seminars, it was postulated that Jews and Germans calculated in different ways. A German, in order to subtract $^3/_3$ from $7^1/_4$, would subtract $^1/_4$ from both fractions and would then calculate $7 - {}^1/_2 = 6^1/_2$. A Jew, on the other hand, would follow a more formal and less concrete procedure. reducing the numbers to a common denominator and then subtracting in order to obtain $^{26}/_4$. For further details also on what follows see Rowe 1986.

28) This thesis is substantially argued in Rowe 1989. On Weimar culture and science see Gay 1968, Forman 1973.

predominated and the group he led gradually distanced itself from the Faculty of Philosophy, of which it was nominally part and which was dominated by conservative and nationalistic positions typical of certain sectors of German university teaching. Very soon Göttingen attracted scholars from different backgrounds, above all from abroad, including many Jews. Their stay at the University of Göttingen left an indelible mark on the mathematicians and physicists who visited there, including, of course, von Neumann, one of the many Jewish researchers who worked there, as well as von Kármán, the Swiss Paul Bernays, the Ukrainian Alexander M. Ostrowski, the Croat William (Willy) Feller and the American (albeit of Russian parents) Norbert Wiener.[29]

The young von Neumann thus found at Göttingen an open, tolerant and culturally broad-minded atmosphere, where he came into contact with a working style that was to form the basis of what is today still considered the way mathematical research should be carried out. This style was created mainly by Klein and was retained even after his death in 1925. Direct communication among mathematicians during scientific seminars and meetings took on great importance, together with the activity of publishing books and papers. Not only did the number of *Privatdozenten* in mathematics increase, but also the number of mathematics students, and refresher courses were organized for secondary school teachers. An extraordinary range of material resources was available to the mathematics community, including a splendid collection of geometrical models and scientific instruments, as well as a rich library organized on a free access basis for the purpose of stimulating informal contacts among researchers.

Since the beginning of the new century, the emphasis placed by Klein on the relationship between mathematical thinking and the real world was replaced by a conception of mathematics as something highly abstract, which became the most salient feature of Göttingen's contribution to the history of this discipline. In actual fact, this transformation was not at all in contradiction with that emphasis but rather represented a more modern and functional aspect of it.

2.2 Hilbert's mathematical optimism

At the turn of the century, the mathematics community was showing a growing interest in abstract structures and the axiomatic approach. The field in which these new tendencies emerged was above all that of algebra. Mathematicians like Richard

29) Among those attending Göttingen and already cited we may mention also the names of other eminent scientists such as Hermann Minkowski, Carl D. Runge, Edmund Landau, Karl Schwarzschild, Ludwig Prandtl, Peter J. Debye, Johann E. Wiechert, Paul A. Gordan; and also Max Noether, Hermann Weyl, Arnold Sommerfeld, Constantin Carathéodory, Gustav Herglotz, Hans Lewy, Erich Hecke, Max Born, Richard Courant, Otto Blumenthal, Ernst Zermelo, Paul Koebe, Robert K.E. Fricke, Otto Toeplitz, Ernst Hellinger, Felix Bernstein, Emmy Noether. To give some idea of the relative weight of the Jewish component suffice it to say that among the latter, Minkowski, Landau, Schwarzschild, Gordan, the Noethers, Weyl, Lewy, Born, Courant, Blumenthal, Toeplitz, Hellinger, Bernstein were Jews.

Dedekind, Heinrich Weber and Ernst Steinitz made a fundamental contribution to the consideration of abstract numerical fields which generalized the properties (or "axioms") characteristic of the "concrete" fields of real or complex numbers: in this way, algebra enlarged its horizons, from the classical issue of solving algebraic equations to the general examination of algebraic structures (such as groups, fields, rings, vector spaces). Set theory – above all due to the work of Georg Cantor – was another of the sectors of development of the new abstract methods. Klein held reservations similar to those of Poincaré on the subject of these developments. Hilbert was instead a fervid supporter of the new trend. His early research on the theory of invariants already reflected the abstract axiomatic approach. The original theme of this theory, which had been carefully cultivated during the nineteenth century, was an interest in the properties of invariance of figures when certain geometric transformations of space are applied: the simplest example of "invariant" is given by the length of a segment when the spatial transformations considered consist merely of rotations and translations. The algebraic study of these problems had become a true testing ground for the computing ability of those trying to solve them and demanded the use of extremely complex computational algebra techniques. Hilbert first pursued and then abandoned this computational approach, deciding to use abstract and purely logical methods of proof instead. The use he made of the principle of the excluded third, or *reductio ad absurdum* argument, in the proof of existence was by no means taken for granted by many mathematicians, who preferred a direct constructive approach to account for the concrete existence of the entities under examination. In this way, Hilbert's approach acted as a stimulus to the discussion of the nature of mathematical entities, the methods of proof and the function of logic in mathematics. He also supported Cantor's research on set theory and transfinite numbers, which led to the same kind of issues regarding the foundations of mathematics and radically changed the mathematical horizon. He also extended his abstract approach to a wide variety of mathematical topics, ultimately proposing a global renewal of mathematics, as a unified system of knowledge.

Hilbert was thus expressing an optimistic view of the tendency towards abstraction and the use of pure logical reasoning – a tendency that, at the end of the nineteenth century, was instead considered by many mathematicians as an actual factor in a crisis that demanded a radical reappraisal of the status of this discipline. The factors underlying this pessimism were thus the same as those reinterpreted by Hilbert in an optimistic and dynamic sense: the indiscriminate use of abstract concepts of which the means for "constructing" them or describing them concretely are not known; the strengthening of the relationship between mathematics and logic and the simultaneous weakening of the traditional relationship between mathematics and spatial intuition, and thus between mathematical truth and intuitive certainty. Of course, this process had remote origins, like the spread of research on non-Euclidean geometry – those spatial configurations for which the Euclidean postulate of parallels does not hold – which had so severely shaken the age-old idea of a close connection between physical space and Euclidean geometry. However, one of the most significant consequences of the weakening of the relationship between mathematics and the real world

was precisely the growing importance of logical and abstract reasoning in mathematics, which itself acted as a stimulus on the process of the mathematization of logic and raised the issue of the relationship between logic and mathematics.

Hilbert intervened vigorously in this debate, which raised deep concerns regarding the future of mathematics. One of his most influential contributions was his book *Foundations of geometry* (Hilbert 1899), in which he presented a re-foundation of the deductive method of Euclid's *Elements* by grounding Euclidean geometry on the *axiomatic* method. Euclidean geometry (as well as other possible *geometries*) was based by Hilbert on consideration of a set of elements that are not defined *per se* but have only to allow an *a priori* verification of certain principles or axioms, starting from which the entire theory can be derived by means of a logical-deductive demonstration procedure. Consequently, the attempt to define an intuitive idea of the fundamental concepts of Euclidean geometry, such as the point, the straight line or the plane, was abandoned as useless: all that had to be done was to define the relationships that necessarily exist between these types of objects when considered in abstraction. This emptying of the theory of all its concrete *content* in favour of the role of the *properties* of the elements defined by the axioms, was expressed by Hilbert in a much quoted remark that, in the axiomatic presentation of Euclidean geometry, the words "point", "straight line" and "plane" could be replaced by the words "chair", "table" and "beer mug" without detriment to the logical truth of the theory. The key point of this program was the need to demonstrate the "consistency" of the axioms, that is, the absence of any contradictions among them: since the aspect of a link to reality had been disregarded as insignificant, only consistency (or coherence) could guarantee the truth and the meaning of the theory.

In a historical lecture given during the International Congress of Mathematicians held in Paris in 1900 (Hilbert 1902), Hilbert announced a list of twenty-three problems, several of which were classical, plus others identified by him, which in his views should form the subject matter of research by twentieth century mathematicians. The first two problems were linked to the question of the foundations of mathematics and reflected not only the importance he attributed to it, but also his confidence that it was possible to resolve such a confused situation. This confidence was based on his intimate conviction of the existence of a pre-established harmony between mathematics and physical reality which led him to conceive of mathematics as the foundation of all the exact scientific knowledge of Nature (Hilbert 1931). In this he was relating back to Klein's approach and so his axiomatic view not only did not contradict but, as we saw earlier, actually represented a development of the emphasis Klein laid on the relationship between mathematics and the real world: it was precisely the existence of a pre-established harmony that acted as a basic premise allowing the axiomatic emptying of mathematics of all real content. Furthermore, this concept had been seen to receive concrete expression in the everyday practice of the Göttingen school, in mathematicians' interest in physics problems and in the close collaboration between mathematicians and physicists. Indeed one of Hilbert's most important research topics – the sixth problem stated in the Paris lecture – was the axiomatization of the principal physics theories, such as the kinetic theory of gases,

which was later extended to cover the new physical theories that emerged at the beginning of the century. Hilbert was thus simultaneously asserting the universality of mathematics and its profound unity.

In the early twentieth century, mathematics was undergoing increasing fragmentation and specialization, apparently as a result of the spread of the abstract axiomatic approach whereby it was uncoupled from its ancient empirical unified reference; at the same time, the relationship with physics and the other natural sciences was no longer determined by the ancient link between mathematics and mechanics. Yet Hilbert actually used the new trends in mathematical research as a starting point for a process of reunification capable of revealing the intimate unitary structure of mathematics and of science in general. He thus pointed the way towards preserving the classical yearning for the unity of science in the new processes of development of individual branches. In this sense, Hilbert represents one of the last great scientists (on a par with Einstein) who were inspired by a unitary view of science and were capable of embracing the whole of it with a single glance. Von Neumann, whose philosophy of science owed so much to Hilbert's teaching, was to become one of the few twentieth century mathematicians who succeeded in retaining this global grasp of the many branches of mathematics, as well as the traditional view of the role of mathematics as a conceptual tool for analyzing the real world and for controlling real phenomena.

Hilbert had such great faith in the usefulness of the axiomatic method to organize single mathematical theories that it occurred to him to derive from it an approach aimed at axiomatizing mathematics as a unitary system, that is, to demonstrate its internal logical consistency in a global sense. This is the essence of Hilbert's "formalist program", which we shall deal with in the following section.

2.3 The problems of the foundations of mathematics and the axiomatic approach

In his 1899 book on the foundations of geometry, Hilbert had reduced the problem of consistency of geometry to that of consistency of the set of real numbers, that is, to state the issue intuitively, of the numbers that are associated with the infinite points of a straight line. Thanks to Cantor, Dedekind and Kronecker it was found that the consistency of real numbers is a consequence of the consistency of arithmetic, that is, of the consistency of the system of axioms that define the set of natural integers: 0, 1, 2, 3, ... Such a system of axioms had been proposed by the Italian mathematician Giuseppe Peano (Peano 1889): it was founded on the hypothetical existence of a distinguished number (zero); on the postulated existence of the successor, that is, the "consecutive" of each number; and on the postulate that, if a set of numbers contains zero and if, by containing a number, it contains also its successor, the latter set will thus contain all the natural numbers.[30] Thus, the mathematical research that

30) This postulate is now known in mathematics as the principle of finite induction.

developed towards the end of the nineteenth century led to the conclusion that the key to a "good foundation" for the whole of mathematics lay in consistency of the set of basic numbers of mathematics, those linked to the elementary operation of counting, and of which Kronecker said they were the "work of God", while all the rest of mathematics was the work of man. From the point of view of Kronecker, the forerunner of the "intuitionist" school of mathematical philosophy (which we shall discuss later), it was absurd to raise the problem of the logical or axiomatic coherence of these numbers. Hilbert thought differently, and indeed raised this very problem (the second on his list) at the Paris conference in 1900.

Hilbert began to concern himself with this topic as early as 1899[31] but then set it aside to concentrate on the study of integral equations and mathematical physics issues. However, the debate was now under way and soon focused on an even more basic level of the "new" structure of the edifice of mathematics – set theory. The discovery of paradoxes or contradictions in set theory by Bertrand Russell, Jules Richard and Cesare Burali-Forti had led to further debate on the validity of set theory.[32] According to Russell and Alfred North Whitehead, the origin of these paradoxes was related to the attempt to define an object in terms of a class of objects containing the object to be defined. This was the kind of paradox discovered by Russell in 1902: if we allow the set-theoretic notion of "set of those sets that do not contain themselves as elements", we find that such a set both *is* and *is not* an element of itself. Indeed, if it is not, in so far as it is a set of all the sets that do not contain themselves as elements, it is; and if it is, insofar that it contains only sets that do not contain themselves as elements, it is not. Zermelo pointed out that even classical mathematical analysis uses this kind of notion – for instance, in the concept of the lower limit of a numerical sequence – which showed that it was not consistent either.

The strategy adopted by supporters of the axiomatic method was to propose the axiomatization of set theory which, by eliminating the inaccuracy of the non formalized language used by Cantor, would avoid emergence of contradictions. This programme was tackled by Ernst Zermelo, after his arrival in Göttingen in the early years of the century. Taking as his model Hilbert's axiomatization of geometry, Zermelo proposed a system of axioms that defined sets using restrictions that would allow the onset of antinomies to be avoided and, at the same time, such as to incorporate all the objects of mathematics and to provide it with a correct logical foundation (Zermelo 1908). Subsequently, Abraham Fraenkel further developed Zermelo's axiomatic system.

31) In an 1899 article, later republished as Appendix 6 to the seventh edition (1930) of the *Grundlagen der Geometrie* (Hilbert 1899), Hilbert had attempted to enunciate an axiomatic system for the set of real numbers.

32) A full examination of this set of issues may be found in Casari 1964, Feferman 1993, Ferreirós 1999. Cantor was well aware of the fact that the "set" of all cardinal numbers or the "set" of all ordinal numbers are contradictory notions: Cantor's letters to Hilbert show this as well as does his exact distinction between the transfinite and the absolute infinity In 1884 already, he calls the totality of all ordinal numbers "ein Symbol des Absoluten" (we are grateful to an anonymous referee for this remark). A rich review of recent research and selected bibliography can be found in Grattan-Guinness 1994, part 5, "Logics, set theories, and the foundations of mathematics".

However, these early attempts to solve these difficulties axiomatically in no way met with the unanimity of mathematicians and mathematical philosophers regarding the approach to follow. Indeed, the controversy was further fanned at the beginning of the 1920s. Russell represented the logicist point of view, the heir to the ideas of Frege, who proposed the need to perfect classical logic and to refound mathematics on this new unassailable base. In open opposition to this program to "salvage" mathematics was the intuitionist trend represented by Poincaré but, in an even more radical fashion, by the Dutch mathematician Luitzen E.J. Brouwer. In the latter school of thought, the existence of a mathematical entity cannot consist in its mere logical possibility but in its actual *construction*. The only acceptable proof of existence in mathematics is that which leads to the constructive definition of an entity and the proof that it verifies the required properties. Quite the contrary, the proofs of existence achieved by reductio ad absurdum (that is, showing that the assumption that no entities exist which enjoy the required properties leads to a contradiction) are to be rejected as purely formal. This implies the rejection of a basic principle in classical propositional logic, namely the so-called *principle of the excluded middle*, according to which, given a proposition P, either P is verified or *non* P is verified. In this way the constructivist approach, which has persisted to the present time, albeit in an increasingly smaller group of mathematicians, questioned the validity of logical demonstration as a foundation for the certainty of mathematical results.

Von Neumann, who, in his first publication on set theory (Neumann 1923) had tried to introduce Cantor's transfinite cardinals more rigorously but not axiomatically, became a convinced supporter of this approach. The first research he developed on the axiomatisation of set theory was contained in an article we have already referred to in the preceding chapter, published in 1925 in the journal of Fraenkel (Neumann (von) 1925). Therefore, he had already autonomously arrived at an acceptance of the axiomatic approach which could only make Göttingen and Hilbert the ideal place and interlocutor for helping the progress of his research. In the work published in 1925 with Fraenkel's help, the still very young von Neumann proposed an axiomatic system for set theory that had been further improved with respect to that of Zermelo-Fraenkel. It was based on the distinction between "classes" and "sets". The idea was to consider a new group of entities – classes – which had the peculiarity of not being able to be included in other sets or classes, while sets are particular classes that may be the elements of a class. By making this distinction it was possible to eliminate many antinomies like that of Russell.

At the same time this research gave von Neumann the opportunity to declare quite openly his acceptance of the axiomatic approach over any other alternative approach[33]. The logicists and the intuitionists, he asserted, had both obtained important results, the future developments of which nevertheless appeared destructive. Indeed they had failed to "rehabilitate" the set theory which, in his opinion, was not only valid but above all essential for the purpose of putting the foundations of mathemat-

33) In the meantime Steinitz's axiomatic theory of fields (1910) and Felix Hausdorff's axiomatic theory of topological spaces (1914) had been developed.

ics on a solid footing. Von Neumann held that Russell, Weyl and Brouwer had stopped short of a penetrating criticism of the elementary logic used up to that time, without opening up new prospects for set theory. His criticism was directed also at Gyula König and he consequently distanced himself from the latter's philosophic approach, although König had been concerned with the problem proposed by Hilbert of working out suitable methods for determining the consistency of axiomatic systems and, in his analysis of antinomies, had introduced considerations regarding the concept of "belonging" in set theory which had certainly influenced von Neumann. In his work, von Neumann presented a logically correct exposition, «in the meaning so far given to this expression in mathematics», and not in the sense of the intuitionists. The spirit of the axiomatic approach was effectively described: «by "set" is meant here something of which it is not known or desired to be known more than what may be inferred from the postulates» (Neumann (von) 1925, p. 219). On the other hand, he was aware that his work did not provide a proof of consistency, because this case could not be reduced, as Hilbert had done for geometry, to the consistency of arithmetic or that of real numbers; moreover, a direct proof, in view of the fact that «in logic much still remains to be clarified», would have been in its turn liable to criticism.

Von Neumann also made explicit reference to the recent work of Hilbert, who, in the 1920s, after fresh controversies had arisen, had come back and concerned himself with the problems of foundations and of mathematical logic. The radical idea formulated by Hilbert to resolve once and for all the endless diatribes was to use the axiomatic method to demonstrate coherence of the *corpus* of mathematics as a whole. Axiomatics thus emerged from a context of particular mathematical theories in order to measure itself with mathematics as a global and unified system. This program, later known as "Hilbert's formalist program", was developed by him together with his principal collaborators Wilhelm Ackermann, Paul Bernays[34] and von Neumann himself. Hilbert's program was aimed at creating a theory of proof that could be used to demonstrate the coherence of any formal system – a metamathematics. In rough outline, the suggested approach was as follows. It was proposed to subject to formal treatment any mathematical theory by means of an axiomatic theory T, using a given formal language L. This language is made up of a stock of basic symbols that could be used together with a list of rules indicating how to construct correct formulae or statements consisting of a finite number of symbols. Some of these formulae are axioms of the theory T, including some general statements of a logical type. Furthermore, rules of inference are given in order to construct formal proofs of new formulae or statements on the basis of the axioms: the formal proofs themselves are actually finite successions of formulae. Given a formula f and a proof β, it can always be decided whether β is or is not a proof of f. T is consistent if it is impossible, starting from T, to form any contradiction (that is, the simultaneous occurrence of an f and the negation of f). Hilbert suggested that, by using these "finitary" methods (in the sense

34) In collaboration with Ackermann, Hilbert published *Grundzüge der theoretischen Logik* (Hilbert, Ackermann 1928), and in collaboration with Bernays *Grundlagen der Mathematik* (Hilbert, Bernays 1934–39).

that only finite successions of formulae are used that are themselves finite successions of symbols of L) it would be possible to obtain metamathematical results that were acceptable also within the most rigid intuitionistic positions.

In 1927 von Neumann published an overview of Hilbert's theory, which included the results obtained by Ackermann and Bernays and several contributions to problems that were still open (Neumann (von) 1927). The ultimate aim was certainly to arrive at a proof of the consistency of arithmetic from a metamathematical point of view. But the young Hungarian mathematician was now fascinated by other research topics, particularly the axiomatization of quantum mechanics – on which he published in 1927–1929 several articles written in collaboration with Hilbert, Nordheim and Wigner.[35] Thus, he later shifted his attention away from questions of mathematical logic, although without losing interest in Hilbert's programme. In September 1930 he thus attended a congress on the epistemology of the exact sciences held in Königsberg (now the Russian Kaliningrad), under the auspices of the Ernst Mach Association and the Berlin Empirical Science Society and upon the initiative of several members of the Vienna Circle. At this scientific meeting the three positions – logicistic, intuitionistic and formalistic – confronted each other on the foundations of mathematics, and were represented respectively by Carnap, by Arend Heyting, a disciple of Brouwer, and by von Neumann himself: their lectures were published in the journal *Erkentnnis*, which had recently been founded by Carnap and was later to become an important instrument of discussion and dissemination of the ideas elaborated inside the Wienerkreis. In his address, von Neumann spoke out in favour of Hilbert's point of view, defending the practice of professional mathematicians against the attacks of those who wanted to restrict their methods and field of action (von Neumann 1931). He presented Hilbert's metamathematics as a pathway to reconciliation with the intuitionist position: to this end, it was sufficient to distinguish the content of a classical mathematics theorem – which it was not always possible to prove using finitary or constructive procedures – from the formal way in which this theorem was derived and by means of which such a verification was possible.

However, the course of events changed radically during the Congress itself. A young logico-mathematician, Kurt Gödel, announced a result that, in its final version, complete with all the proofs and implications, was published the following year (Gödel 1931; see Zach 2005). Gödel claimed that if T is any formal theory presented in finitary form, *à la* Hilbert, which includes an axiomatic formalization of arithmetic (e.g., that given by Peano's axioms), the consistency of T cannot be demonstrated using methods that can be formalized within T, unless T is inconsistent. This result was actually a corollary to the theorem known today as Gödel's Incompleteness Theorem, according to which, given a theory T of the above type that includes arithmetic, there always exists an undecidable arithmetic proposition, that is, one such that in T the proposition and its negation occur simultaneously. In conclusion, Gödel's results demonstrated the impossibility of obtaining a complete proof of the logical coherence

35) Let us recall, in particular: Hilbert, Neumann (von), Nordheim 1927; Neumann (von), Wigner 1928a, 1928b, 1928c, 1929a, 1929b; Neumann (von) 1929a.

of mathematics in the contexts of the rules laid down for metamathematics and dealt a death blow to Hilbert's programme. Furthermore, it seemed to vindicate those who, like Poincaré, considered it a pipedream to want to justify arithmetic by means of an axiomatic foundation and clung to a position of Kantian inspiration according to which the human mind possesses an intuitive idea of natural numbers that precedes all formalization.[36]

In actual fact, during the Königsberg Congress, none of the eminent participants realized the full import and implications of the result announced by Gödel – with one exception: von Neumann. After the discussion the latter rushed up to Gödel and took him aside in order to get a better understanding of his demonstration. He then left the Congress in a state of extraordinary excitement and spent the next month working on the issue. Less than two months later he wrote to Gödel to announce he had demonstrated, as a consequence of the theorem of incompleteness, that the consistency of arithmetic cannot be proved. Gödel replied that he had in the meantime succeeded in obtaining this demonstration and sent him a copy of the article that had already been presented for publication.

After the publication of Gödel's results, von Neumann definitively abandoned his work on mathematical logic, even if maintaining the strong belief that logic should play a key role in mathematical praxis.[37] Gödel's result was perceived by von Neumann as a crushing defeat that left visible traces on his researcher's psychology and influenced his conception of the limits of mathematical rigour. But nevertheless, he continued to believe firmly in the epistemological value of mathematical practice based on a formal logical and axiomatic approach. From this point of view, faith in the value of the axiomatic method freed from the constraints of the Hilbertian formalist programme became the linchpin of von Neumann's mathematical thinking. He thus became one of the principal actors in the "pragmatic" reconstruction of mathematicians' confidence in the value of their discipline. Moreover, his work on axiomatization and on proof theory led to a view of mathematics as «a combinatorial game played using primitive symbols in which it had to be determined in a finitely combinatorial way which combinations of primitive symbols the methods of construction or 'proof' led to», as he claimed at the Königsberg congress (Neumann (von) 1931). He never abandoned this view, and indeed built it up over the years, and this view helps to explain his interest in the scientific topics he concerned himself with in the 1940s and 1950s.

In the following chapter we shall endeavour to summarize von Neumann's scientific thought, which had already acquired a mature form on the eve of his depar-

36) According to Poincaré, the principle of finite induction is a typical example of a synthetic *a priori* judgment in the Kantian sense, which – in view of the central nature of this principle in mathematical proof procedures – demonstrates that mathematics is not a purely logical-deductive science (Poincaré 1902).

37) A *Leitmotiv* of his scientific work was the importance of understanding and unveiling the logic underlying any scientific and technological problem; see for example his 1936 work (in collaboration with Garrett Birkhoff) on the logic of quantum mechanics and his 1956 work on probabilistic logics and automata.

ture for the United States. At this point, however, it is necessary to emphasize an extremely important point. There is some confusion in the history of science literature regarding axiomatics and Hilbert's formalist programme. Axiomatics was a vast movement of mathematical research – a trend in mathematical research practice, if one prefers – which took shape during the second half of the nineteenth century also thanks to Hilbert's contribution, but cannot however be reduced to Hilbert's efforts alone. Hilbert followed this programme with an even more ambitious one, although this time of an essentially logical-foundation type, the "formalist programme", the main features of which have been outlined above. Confusing these two aspects can lead to the error of believing that the collapse of the formalist program led to a crisis of the axiomatic approach and opened up the way to a more heuristic, empirical mathematical practice, or one that was more concerned with applications issues.[38]

None of this is true. The collapse of the Hilbertian programme in no way implied a crisis in axiomatic practice. Indeed, axiomatic practice represented a channel leading away from the reefs on which mathematical research had been stranded as a result of the barren conflict between formalism and intuitionism. The axiomatization of the basic theories of mathematics – set theory in particular – formed the protective screen behind which mathematicians could now defend themselves from the monsters represented by antinomies and logical paradoxes and regain confidence in their research. Von Neumann was not only a "victim" of the crisis caused by Gödel's results, but rather one of the main figures in the construction of a new scientific paradigm in which the Hilbertian style global reductionist view was abandoned. This new "pan-mathematical" paradigm was based on the efficacy of the axiomatic method as a concrete research practice and consequently on a view of the use of mathematics based on the concept of "model".

Therefore, the axiomatization of set theory became a symbol of this new trend, as did also another of the fundamental results obtained by von Neumann in those years, again in the framework of the Göttingen research programmes – the axiomatization of quantum mechanics.

2.4 The axiomatization of quantum mechanics and functional spaces

In the early twentieth century the extraordinary burgeoning of research in the field of physics led to a radical change in the fundamental beliefs of physicists and to a profound crisis in classical mathematical physics. It is customary to speak of three "revolutions" occurring in physics in the first third of the century: on the one hand, the emergence of Einstein's special theory of relativity, followed by his theory of

38) The confusion between Hilbert's formalist programme and axiomatics is present in the work of Philip Mirowski (Mirowski 1992, 2002). He does not take into account the history of axiomatics, merely considering Hilbert's formalist program. This confusion distorts the analysis of von Neumann's work, by introducing the consideration in his scientific pathway of non-existent gaps or forced counterpositions between an earlier phase ("purity") and a later one ("impurity").

gravitation (general relativity), which replaced Newtonian theory on a macroscopic scale; and the emergence of quantum theories claiming to account for the phenomena taking place on a microscopic scale. These developments marked the beginning of so-called "theoretical physics" and also led to a turning point in the kind of tools used in mathematical analysis of the physical world: new methods replaced the existing mathematical approach in mechanics and classical mathematical physics, namely the theory of functions, infinitesimal calculus and the theory of differential equations.

At the outset, many mathematicians threw themselves body and soul into the task of preparing mathematical tools suitable for treating the new physical theories. Even though, in the case of Einstein's relativity, physicists could rely on a branch of mathematics that was already developed, differential geometry, the contribution of mathematicians such as Poincaré, Hilbert, and Hermann Minkovsky was decisive in allowing a correct mathematical formulation of the special theory of relativity, derived from Einstein's physical insights, and in opening the way to further developments. The Italian mathematician Tullio Levi-Civita – although admitting he was a "conservative" as regards the principles of mathematical physics – enthusiastically espoused the topics of the new mechanics and made a fundamental contribution to the mathematical formulation of general relativity.[39] The greatest difficulties were encountered in the mathematical treatment of quantum mechanics: this issue represented one of the major research topics in the Göttingen environment and was included within the general framework of Hilbert's scientific project aimed at providing axiomatic explanations of the various mathematical and physical theories.

The theoretical problem posed was how to account for experimental evidence regarding the internal structure and the dynamics of atoms (Jammer 1966; Garola, Rossi 2000). During the first two decades of the century a large body of experimental data had been gathered – some of which was gleaned from studies on radiation – that seemed to be in open contradiction with Newton's laws of motion or with the equivalent mathematical formulations, such as the "variational" formulation given by William R. Hamilton. For example, if the atom was composed of a heavy nucleus surrounded by a group of electrons, according to Ernest Rutherford's model, why was it that the electron did not lose energy and fall into the nucleus, as would seem to be required by the laws of motion? And likewise, how to account for the fact that each type of atom emits or absorbs very specific colours (i.e., light wavelengths)? Between 1925 and 1926 two possible explanatory schemata were presented almost simultaneously. One of these was formulated at Göttingen by Werner Heisenberg, Max Born, and Pascual Jordan, in a purely algebraic form, generalizing the basic variables associated with a particle in Hamilton's classical mechanics formulation: the position and moment of an electron were represented by infinite matrixes, that is, by mathematical objects possessing the property that their product is not commutative. In this way

39) General relativity raised more complex mathematical problems. During the 1910s, Levi-Civita proposed to Einstein several essential modifications of his initial version of the gravitational equations which enabled them to be expressed in their correct invariant form. See Levi-Civita 1919 and De Maria, Cattani 1989.

it was possible to obtain appropriate generalizations of Hamilton's equations. This "matrix mechanics" showed good agreement with the experimental evidence.

The explanatory outline provided by the Austrian physicist Ernst Schrödinger, then professor at Zurich and who succeeded Max Planck at the university of Berlin in 1927, was instead grounded essentially on physical considerations and was based on the idea of the dual "nature" of subatomic particles – simultaneously waves and particles. Indeed, the experimental devices revealed that the particles had now a wave behaviour, now a particle behaviour. Niels Bohr, director of the Copenhagen Institute of Theoretical Physics, had incorporated this phenomenon in a "principle of complementarity", which he believed could be extended to real phenomena in all fields, including biology and psychology, and which delimited the very boundaries of knowledge. According to this principle, elementary particles display two complementary properties, each of which accounts for only a part of the subatomic processes. Further evidence of this principle was the existence of complementary variables, such as the position and moment of a particle, which could not both be determined simultaneously (Heisenberg's "uncertainty principle"). In Schrödinger's "wave mechanics" each electron could be associated with a differential or "wave" equation, the resolution of which, in the case of the hydrogen atom, allowed experimentally predicted wavelengths to be obtained. Schrödinger had also demonstrated that, using his equation, it was possible to obtain the elements of Heisenberg's matrixes, thereby pointing to the basic equivalence of the two descriptions. However, a fundamental difference existed between Heisenberg's and Bohr's statistical interpretation of quantum mechanics and Schrödinger's view, which was supported also by Einstein, who defended the need to reinstate a deterministic description of reality. The physical discussions were linked to the developments of mathematical formalism. The most important problem seemed to be to find a general formulation to accommodate both wave mechanics and matrix mechanics. Hilbert's theory of function spaces, an abstract theory that had been developed without any connection with these physics problems, proved to be the best suited to this purpose.

The investigation of abstract function spaces carried out at the beginning of the century by a number of mathematicians with whom von Neumann had been trained is a good example of the trend towards abstraction that characterized twentieth century mathematics and of which mention has already been made. Indeed, during the nineteenth century, the main object of mathematical analysis was to study functions associated with the several problems of classical mathematical physics (theory of elasticity, heat, potential, vibrating strings), which derived from the attempts to solve the differential equations used to describe such specific problems. Another approach was to use the calculus of variations, in which the solution to the problem was identified as that which rendered a function of this solution a minimum (or maximum or at least an "extreme"). The approach in variational terms – and thus the consideration of "functions of functions" or "functionals", already mentioned in Chapter 1 – was without doubt one which opened up the way to modern functional analysis and removed the limitations imposed on the study of individual functions typical of the classical theory of functions.

In a rough and ready way the new approach may be said to consist in seeking methods that would allow the problems to be solved, not one by one, by studying differential equations and particular functions separately, but starting from a general, overall point of view, reproducing what Hilbert had done for the theory of algebraic invariants. Instead of individual functions, there would be sets of functions ("spaces") subjected to the action of other functions or abstract "operators", given by the equations defining the problem being examined.[40] The representation theorem demonstrated by Riesz and Fischer, showed that several of these sets of functions could be considered as "spaces" in the sense that they could be given a structure similar to that of Euclidean spaces: in function spaces the points represented the functions and each "point" was described by an infinite set of coordinates, that its, by a sequence of numbers. A space of this kind, called a Hilbert space, is thus a generalization of the customary three-dimensional space of Euclidean geometry – or, more generally, of the finite-dimensional vector space of linear algebra – to the case of an infinite number of dimensions. This space was given an algebraic-geometric structure and language, and the linear operators acting on it could in a way be considered similar to the linear transformations of n-dimensional spaces, including their representation by means of matrixes. Furthermore, in this kind of space, it was possible to introduce a notion of measure, of distance, which generalized the Euclidean notion of distance between two points: this made it possible to introduce a topological structure and such concepts as those of continuity and limit. This line of research was thus linked to topology (or *analysis situs*), the object of which is the study of geometric properties that remain invariant when the space is subjected to continuous transformations.

In this increasingly abstract approach, the predominant geometric and/or physically based ideas typical of the nineteenth century were replaced by the predominance of general algebraic and topological concepts. Abstract algebra, to which a fundamental contribution was made in those years at Göttingen by Emmy Noether and other mathematicians – proceeding along the path opened up by Hilbert in invariant theory – provided the panoply of tools needed by the new developments in analysis. Following this trend towards deeper levels in abstraction, the object of linear functional analysis was first function spaces and the spaces of sequences, and later, the general abstract spaces, the points of which could represent any kind of mathematical object. The axiomatic point of view was perfectly well suited to such an approach: it was a matter of identifying interesting properties and of suggesting them as space axioms, without worrying about the nature of its "points".

40) For the sake of example, in order to determine a function $u(x)$ verifying the differential equation:

$$x\frac{du(x)}{dx} + 4x^2 = 0$$

(where $\frac{du(x)}{dx}$ is the derivative of $u(x)$), it is possible to consider the set of all functions $u(x)$ having given properties (such as that of possessing a derivative) and the "operator" (or "functional")

$$L = x\frac{d}{dx} + 4x^2$$

which acts on the set of functions u, and one seeks those for which $L(u) = 0$.

We shall now briefly outline the key idea behind application of Hilbert spaces to quantum mechanics. Heisenberg had already described several quantities of the physical system by means of infinite matrixes. The mathematical idea behind this was that of representing the state of an atom by means of a vector in a Hilbert vector space – given by an infinite number of coordinates, each of which was a complex number – while the observable physical quantities, such as position, moment or energy, were described by means of special linear operators (known as Hermitian or self-adjoint operators) which acted on this space. For those who know a little more about mathematics we shall add that, in the case of the operator representing energy, the eigenvalues of the operator represent the energy levels of an electron and the corresponding eigenvectors represent respective stationary quantum states of the system. The fundamental physical observation in quantum mechanics, according to which a measurement perturbs the system, and the consequent fact that the results of the successive measurements of two observable quantities depend on their order in time corresponds, in this mathematical representation, to the fact that we are dealing with non-commutative operators. Consequently, the Heisenberg uncertainty principle, that is, the impossibility of simultaneously measuring position and moment, was associated with a significant mathematical property. This idea was suggested for the first time by Paul A. Dirac and Pascual Jordan in 1927, showing how the operators corresponded to Heisenberg's matrix representation and how, on this basis, it was possible to reconstruct Schrödinger's wave equation. However, the mathematical formalism they used caused controversy and was not universally accepted.

The axiomatic formalization of quantum mechanics was without doubt a central problem for Hilbert and for Göttingen's scientific circle. As in the case of the foundations of mathematics, concerning oneself with this topic meant riding the crest of the wave. Von Neumann, who was certainly attracted by this kind of challenge, again showed he was equal to the task. The first step he took in this field was to undertake direct collaboration with Hilbert and his physics assistant, Lothar W. Nordheim, who were then endeavouring to simplify and perfect Dirac and Jordan's treatment: this collaboration led to publication in 1927 of a paper on the foundations of quantum mechanics (Hilbert, Neumann (von), Nordheim 1927). In the introduction to the article, it was claimed to be necessary to completely separate the physical interpretation from the mathematical formalism in order to ensure true understanding of the theory. Subsequently, von Neumann decided to enhance the results of this work by using an axiomatic formulation of the abstract Hilbert space concept: the state of the atom – corresponding to a vector in the abstract algebraic formulation – was introduced as an object characterized by the axioms of an abstract Hilbert space.[41] On the strength of this axiomatic construction, in two papers published in 1929 in the German journal *Mathematische Annalen* (Neumann (von) 1929b, 1929c), he devel-

41) Courant has written about the influence of Schmidt on von Neumann's views: «It was the great merit of von Neumann, guided by Schmidt, to see that what matters really is the structural situation in abstract space» (interview of May 9, 1962, Quantum Archives, quoted in Heims 1980, 113). For a deeper analysis of von Neumann's contribution in this area, subsequent developments and a comparison with Wiener's views, see Heims 1980.


HOTEL KAISERHOF.
BERLIN W 8

Dear highly honored Professor,

I had the opportunity several times to speak to Leo Szilárd about the student competitions of the Eötvös Mathematical and Physical Society, also about the fact that the winners of these competitions, so to say, overlap with the set of mathematicians and physicists who later became well-respected world-figures. Taking the general bad reputations of examinations world-wide into account it is to be considered as a great achievement if the selection works with a 50% *probability of hitting the talent. Szilárd is very interested in whether this procedure can be applied in the German context and this has been the subject of much discussion between us. However, since we would like above all to learn what the reliable statistical details are, we are approaching you with the following request: We would like:* 1. *to have a list of names of the winners and runners-ups of the student competitions,* 2. *to see marked on the list those who were adopted on a scientific basis and those adopted for other work,* 3. *to know your opinion about the extent to which the prizewinners and the talented are the same people and, for example, what proportion of the former would be worthy of financial support from the State in order to make their studies possible. Please forgive me for setting such a tiring task but we would be very grateful to you for receiving of the information herein requested or, otherwise, an indication of how such information may be obtained.*

Thanking you in advance, I remain your grateful pupil,

Neumann Jancsi

Figure 2.1 A letter from Jancsi Neumann from Berlin to his professor Lipót Fejér in Budapest (July 17, 1929).
Source and English translation: Marx 1997 (with permission)

oped the general theory of Hermitian linear operators on a Hilbert space of any kind and their eigenvalues (the spectral theory). Again for anyone who knows a little more about mathematics, we add that, from this point of view, both the space L^2 of the measurable and square integrable – in the sense of Lebesgue-measurable – complex-valued functions and the space l^2 of the square summable sequences of complex numbers were special and well-known examples of Hilbert spaces, which corresponded precisely and respectively to Schrödinger's and Heisenberg's mathematical formulations. Moreover, the Riesz-Fischer representation theorem, which had revealed the existence of a one-to-one correspondence between these two spaces, was evidence of the correspondence between the two presentations of quantum theory.

Between the end of the 1920s and the early 1930s, von Neumann published a large number of studies on quantum mechanics, some of them in collaboration with Wigner.[42] He concerned himself in particular with the physical import of the theory such as the concept of measurement and its statistical aspects. This was, in actual fact, the most difficult side of the new discipline. One of the basic assumptions of classical mechanics was a conviction that, in every experimental observation, the error due to possible perturbations caused by the measuring instrument on the observed object could always be reduced: it was thus acknowledged to be possible to construct instruments capable of guaranteeing an objective reading that was independent of the observer performing the experiment. This assumption was now being questioned: Bohr and Heisenberg had shown that it was impossible to obtain an objective description of the real world, because of interactions between the atoms and the measuring instruments, which introduced an element of uncertainty regarding the state of the atom. In their view, the human observer carries out an objective reading of the instrument's signals, but the observation refers to the combination of object and instrument. In this way, any prediction made of the future state of the atom can only be statistical in nature.

This loss of certainty, similar in a way to that occurring in mathematics with reference to the problem of logical consistency, was tackled by several physicists still faithful to determinism. Einstein claimed that several "hidden parameters" existed that, once determined, would eliminate the element of uncertainty. A much more innovative position was held by Wiener, who dedicated himself to the project of rolling back the frontiers of physics and using suitable mathematical tools to tackle the problems in which uncertainty is present, or in which random factors intervene and, ultimately, in which the information is not complete. As for Von Neumann, he developed a strategy similar to that introduced to solve the problem of the antinomies in set theory, namely to use the axiomatic approach to eliminate the source of difficulties of interpretation and to avoid breaking with traditional views in mechanics and physics research.

Thus, both in the case of set theory and the foundations of mathematics and in that of quantum mechanics, von Neumann found in the axiomatic approach a way out of the crisis and a suitable context in which to restore to research a feeling of

42) See note 35.

confidence. And this indicates just how coherent and mature von Neumann's scientific outlook was already in the late 1920s.

Marcello Cini summed this situation up very effectively by defining von Neumann as «the man who quite definitively transformed the retreat that physics had to make with the renunciation of classical determinism into the reassertion of its supremacy, by succeeding in bringing chance back into the laws of logic» (Cini 1985, 104). Indeed, by means of an axiomatic presentation, von Neumann introduced as an internal proposition of the theory the absolutely natural character of quantum mechanics – that is, the impossibility of existence of other equivalent theories with "hidden parameters". At the same time, this formulation enabled him to develop a theory of measurement in which the measuring process had been reabsorbed into the mathematical formalism. In order to pursue this objective, he proposed a bold and controversial break with the conventional distinction made between observer and measuring instrument: in his view, this was an arbitrary distinction as indeed every observation is necessarily subjective. His theory of measurement allowed him to obtain a description of the state of the observed system and of the measuring instrument. The observer's awareness of the result of the observation modifies this state and only in this way is it possible to explain the measurement process. In order to axiomatize this statement, von Neumann assumed that the duration of the measurement lasted only an instant, thus compressing the idea of a process that developed in time within an abstract and atemporal logical scheme. In this way, the subject, the thinking mind or consciousness, became part of the theory in order to expel from the theory the idea of unpredictability or randomness of a physical phenomenon.

This approach of von Neumann's, although restoring "peace of mind" to physics, left open the problem of describing the organization and the dynamics of cognitive processes (Heims 1980, 133 ff.). Many years later, he also addressed this problem, attempting to work out a theory that could be used to describe cognitive processes by means of an "automaton": this theory thus aspired to complete the framework of the mathematical-axiomatic analysis of the real world.

The results of all this research were collected in his famous book on the mathematical foundations of quantum mechanics, published in Berlin in 1932 and completed in 1936 by means of a paper on the logic of quantum mechanics written with Garrett Birkhoff (Neumann (von) 1932a; Neumann (von), Birkhoff 1936). The book became the standard exposition of the principles of quantum mechanics and von Neumann's prestige was further consolidated when his mathematical treatment proved its capacity to accommodate subsequent enhancements of the theory, such as relativistic quantum mechanics and, at least partially, quantum field theory. Von Neumann's formalization had a much more limited influence on research practice – Dirac's presentation was actually more commonly used by physicists in the development of quantum mechanics. Indeed the link between theoretical physics and mathematics, which had seemed so solid at the beginning of the century, weakened, opening up a gradually widening gap, both psychological and cultural, between the two disciplines (Faddeev 1990). It is significant in this connection that von Neumann's book was translated into English only in 1955 – even though the original German edition had been reprinted

in the United States in 1943. Von Neumann himself then abandoned these studies, concentrating on the purely mathematical development of the theory of operators in Hilbert spaces, in the best tradition of the school of Riesz and Hilbert.

2.5 A crucial contribution to game theory

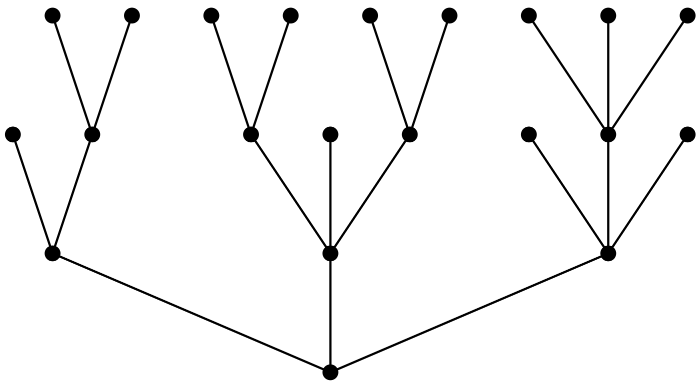
Man's interest in games clearly has remote origins. A much more recent phenomenon is the idea of studying games using scientific methods, in particular mathematics, to determine the best way of winning or at least of reducing losses to a minimum, as well as to gain a theoretical understanding of the structure of specific games. It may well be said that it was during the eighteenth century, a time enthusiastically focused on the recreational and fun aspects of human activities and, at the same time, permeated with a scientific and rational view of things, that the idea arose of studying this order of issues, particularly with regard to parlour games and gambling (lotteries and the like). The preferred mathematical tools for addressing these issues were identified as probability calculus and combinatorial mathematics.

During the nineteenth century the concept of game was appreciably extended, as the idea began to arise that each form of conflict or competition among individuals for an extremely wide range of purposes – economic competition, military clashes, and so on – could be schematized in the form of a "game" in the broad sense. It was in the field of military strategy that a scientific conception of conflict was developed, in particular through the work of the famous Prussian general Karl von Clausewitz. However the rare developments involving the use of mathematical and not merely discursive analysis were limited to a few board games – chess, in particular, until the early twentieth century. The first volume of the encyclopedia of mathematical sciences, published between 1901 and 1904, contained a short article by Wilhelm Ahrens dedicated to mathematical games (Ahrens 1900), although this research still carried little weight and aroused the interest of only a small number of mathematicians. Ahrens' article clearly shows how the aim of the research was still confined to the study, carried out mainly using combinatorial methods, of games such as chess, Nim and cards, especially Patience.

However, in the first few years of the century, the traditional issues raised by games were addressed from innovative viewpoints, which considered various logical, psychological and probabilistic aspects and reflected new trends in mathematical research. Moreover, the study of chess, poker and other games, the playing of which does not depend solely on chance, also lent itself to emphasizing the analogies and connections with problems ranging from military strategy, to ethical reflections on moral choice and behaviour, to the analysis of competitive mechanisms in the social and economic fields. As a consequence, the views of those who developed the scientific analysis of games out of a pure interest in mathematics and logic tended to interact with that of persons interested in their applications to economics, sociology and military science.

The most significant result marking the renewed interest in games based on a sophisticated mathematical approach is represented by Ernst Zermelo's theorem on chess, which was presented at the International Congress of Mathematicians held at Cambridge in 1912 (Zermelo 1913). It followed an axiomatic approach and exploited set theoretic methods.

Zermelo's original idea was to describe a game by means of a tree structure, also known as the "extended form" of the game. The following figure gives an idea of the extended form representation: the various configurations that may be taken up by the game are represented by "nodes" and the moves leading from one configuration to another are represented by arrows.



The extended form representation has as its starting point the initial configuration or "root" of the game. For example, in the case of chess, the root is given by the configuration in which all the pieces are found in their initial positions (the initial node in the figure). Starting from this initial position, the first player can make a certain number of moves which are all marked on the tree diagram and, in the case of chess, are obviously much more than the three shown in the figure. Then all the possible moves available to the second player are marked and so on. The game can therefore be described by a graph having a tree-like structure since it does not contain cycles (the branch of a tree never comes back to itself) and is connected (no branch is isolated because it springs from another branch). The tree-shaped graph represents all possible variants of the game itself. It is easy to appreciate that the extended form of a game such as chess is of an enormous complexity and can in no way be drawn, and this is true for the majority of games. Nevertheless, Zermelo invented a procedure called "retrospective induction" consisting in the analysis of the tree structure by tracing back to the root ends, which allowed him to prove his theorem. In essence, his result asserts that there is a "rational" behaviour (or strategy) of the players such that the outcome of a game of chess is determined. In other words, since there are only three possible outcomes – white wins, black wins or a stalemate ensues – there is a strategic behaviour which inevitably leads to only one of these outcomes, even though we do not know which of the three it is. Indeed the limit of Zermelo's theorem is that it is a theorem of existence, non-constructive, the typical theorem that is un-

acceptable to constructivists *à la* Brouwer.[43] Furthermore, the problem remains open of defining what rational behaviour is, although the most obvious way of conceiving it is to imagine that it satisfies the criterion of allowing the best possible result to be obtained, one of course that is compatible with the fact that the adversary has a similar goal. Zermelo's theorem ultimately states that if the two players adopt a "rational" behaviour, the outcome of the game is predetermined and therefore, in principle, the game of chess is trivial.

It is important to note that Zermelo's approach was aimed at determining the solution or "value" of the game on the basis of purely objective and mathematical criteria, without making use of subjective criteria of a psychological nature.

The French mathematician Émile Borel addressed the same kind of issues in a series of articles published from 1921 on. These articles were preceded by others on the applications of probability calculus, which may be considered to have been preparatory. Indeed Borel considered as fundamental the link between the calculus of probabilities and the players' strategic choices, as he believed that probability was the best way to describe what he called the "player's skill".[44] He therefore followed an approach that was diametrically opposed to the axiomatic approach, taking as his starting point an analysis of the psychological factors ignored by Zermelo.

One extremely important aspect of Borel's research was the representation of games by means of what today is denoted as the normal or strategic form. We shall give a simple description that is closer to the modern treatments but equivalent to that adopted by Borel and then by von Neumann. The idea is to list all the possible strategies that each player may adopt. In the case of chess, a strategy represents a predetermined sequence of moves that the player can adopt in a game: evidently the number of these strategies is very high. In the case of two players we thus have two sets of strategies: $\{s_1, \dots, s_n\}$ for the first player, $\{t_1, \dots, t_m\}$ for the second player. Corresponding to the choice of each pair of strategies (s_i, t_j) the game has an outcome that may be represented by a numerical value: for instance, in chess a win by white may be represented by the value 1, that by black by -1, and stalemate by 0. Chess is an example of a "zero sum" game, that is, one in which a win by one player is equal and opposite in sign to that of the second player. We shall now limit ourselves to considering games of this kind.

A table may thus be constructed – which is denoted as the strategic or normal form – in which the rows contain the strategies of the first player and the columns those of the second one, and in the boxes the wins of the former (which identify, by changing the sign, also the losses of the latter, since it is a zero sum game).

43) Let us take the classic game of "noughts and crosses". The game consists of taking turns to place a "nought" or a "cross" in one of nine squares in a grid with the goal of forming a line, column or diagonal of one of the two signs. It is easy to see that both players adopt a strategic behavior that inevitably leads to a drawn game. The situation is similar in chess, except for the fact that we do not know what the "inevitable" solution is. This kind of game is said to have a "value" and in a sense is of no interest as its solution is predictable. Chess may be said to retain its interest as we do not know what the optimal solution is nor the strategic behaviours that lead up to it.

44) For a complete bibliography of Borel's numerous publications on the topic and an analysis thereof see Dell'Aglio 1995. Here we shall merely cite Borel 1921, 1924, 1927.

Let us take a well-known example, namely that of the game of Chinese morra (rock, scissors, paper game) in which two players simultaneously show a "rock", "scissors" or "paper". According to the rules of the game the stone beats the scissors, the paper beats the rock and the scissors beat the paper, while the simultaneous appearance of two identical objects ends in stalemate. Each of the two players therefore has two possible strategies ($s_1 = t_1 =$ "rock", $s_2 = t_2 =$ "scissors" and $s_3 = t_3 =$ "paper"). By assigning the value of 1 (respectively – 1) to a win by player one (respectively to a loss by player two) and 0 to a stalemate, the following normal (or strategic) form can be constructed, which is also called its "payoff" matrix:

	t_1	t_2	t_3
s_1	0	1	-1
s_2	-1	0	1
s_3	1	-1	0

As soon as the strategic form has been determined, the problem arises of examining what possible game outcomes there are. For this purpose it is necessary to formulate hypotheses concerning the player's behaviour, that is, to attempt to give a description of what we might call his "rational" behaviour. The idea that surfaces in Borel's work – and which was then taken by von Neumann as an axiom – is that the player might follow a prudent path in view of the fact that he is facing an opponent who is pursuing a similar goal and therefore he cannot reasonably hope to obtain what would be the best result in his eyes. He could therefore act prudently or "modestly". For each strategic choice he will observe what is the worst result that he can achieve depending on the strategic choice made by the other player: this amounts to considering for each row the minimum payment value. He will therefore opt for the lesser evil, choosing the greatest among these minima (the maximum of the minima, or "maximin") and adopting the corresponding strategy. The other player should also behave likewise: taking account the opposite value of his payments, he will seek the minimum of the maxima, or "minimax". If minimax and maximin coincide, the game will allow of a solution, which corresponds to the optimal behaviour of the two players – to obtain the maximum possible compatible with a symmetrical behaviour of one's opponent.

It is easy to see that Chinese morra does not allow of a solution of this kind. Conversely, in several cases, there is a minimax solution. Let us consider, for example, the normal form of a hypothetical game with four strategies per player, as represented in the table below. It may be immediately seen that in this case the minima on the rows is $\{-15, -10, -9, -12\}$, so that the maximin is -9. On the other hand, the set of maxima on the columns is $\{6, 12, 8, -9\}$, so that the minimax is also -9. (See the table on top of the next page.)

However, it must be acknowledged that this is just a fluke. What can be done in the case of games that do not allow of a solution like Chinese morra? Also in this case, Borel put forward a crucial idea. The idea was to allow the player not to make use of

	t_1	t_2	t_3	t_4
s_1	−10	0	4	−15
s_2	4	12	1	−10
s_3	0	1	8	−9
s_4	6	11	−11	−12

the available strategies with the same propensity. In order to represent this different consideration of the various strategies it is necessary to probabilize the strategic form, in the sense that the various strategies are adopted with the respective probabilities $p_1, \ldots, p_n$ for the first player and $q_1, \ldots, q_m$ for the second player. These are the so-called "mixed strategies" (reserving the name of "pure strategies" for those that are not probabilized). Correspondingly, the payoffs are weighted as a function of the probability with which the strategies are implemented. The attribution of a probability to the various strategies was, in Borel's idea, the reflection of the player's psychology and of his personal ability.

As early as 1921 he realized that the use of mixed strategies allowed many games to be resolved in the sense that this use meant that minimax and maximin could be made equal. In other words, Borel managed to demonstrate, in a series of special cases, the theorem that – after the publication of von Neumann's work on this subject – was known as the minimax theorem. However, Borel ruled out that any such theorem could be true in general, or that for all two-player zero-sum games the equality between minimax and maximin was valid in mixed strategies. Borel, even when he realized that in a series of cases that he had considered negative the theorem was valid, continued to adopt a generally negative attitude, in the belief that for a very large number of strategies it would be possible to demonstrate its falseness. Such an attitude is only apparently odd. Indeed Borel's theory of games was aimed at dynamically analyzing the player's psychology and not at ensuring a mechanically predetermined solution to the game.

Von Neumann's approach in this field took the opposite direction, that is, the search for a general solution within the framework of an axiomatic formulation. Let us defer for the time being the analysis of why and how he decided to take an interest in game theory, in order to give a summary description of the results he obtained and to characterize the conceptual aspects. It is a known fact that he presented the first results on the theory of parlour games to the Göttingen Mathematical Society in December 1926, when he was still a Rockefeller Foundation junior fellow. Towards the middle of 1927 he sent the journal *Mathematische Annalen* a manuscript that was published in 1928 (Neumann (von) 1928b).

In this work he examines a zero sum competitive game between two persons and presents it in a mathematical formalization in which attention is focused on the strategic options of each player. As in the measurement process in quantum mechanics, time is considered as wholly compressed into the instant in which the game is

resolved. Therefore, it is not the dynamics of the game that is studied, but the conditions required to ensure existence of a solution in which there is a compatibility between the players' goals aimed at minimizing losses and optimizing benefits, allowing oneself to be guided exclusively by a criterion of prudence. Furthermore, the players are conceived of as omniscient subjects, capable of knowing all their own possible strategies and those of their opponent, and of calculating the outcomes.

The central result of this paper was to provide a general demonstration of the minimax theorem, naturally for mixed strategies, and his true masterpiece was represented by the brilliant and wholly original demonstrative technique. The latter was based on seeking the solution to a given system of equations and inequations which was reduced to a topological problem. This allowed von Neumann to bring in Brouwer's fixed point theorem. This theorem, demonstrated by Brouwer in 1910 (Brouwer 1910), claims that, if we take a bicontinuous one-to-one transformation (a "homeomorphism") of a solid sphere (or a ball) in itself, there exists at least one point on the ball that is transformed into itself, i.e., that remains "fixed" in the transformation. The demonstration of the existence of the minimax was reduced by von Neumann to a demonstration of the existence of a fixed point, which made it possible to draw on considerations that were relatively akin to Brouwer's theorem. The demonstration technique was the starting point for many subsequent developments and the use of fixed point theorems became a standard technique in a large number of applied mathematics fields, in particular in mathematical economics. Ten years later, the relation between this proof and convex set theory was grasped and the minimax theorem was demonstrated in a much simpler and more direct fashion on the basis of this theory (Ville 1938), which allowed its field of application to be extended even further.

The 1928 paper gave rise to a new game theory in axiomatic form, which distanced itself from Zermelo's approach in that it used the much more efficient approach of the concepts of strategic form and pure and mixed strategies, and from that of Borel regarding the abstract and rationalist view that had pushed von Neumann in the opposite direction to that of the French mathematician, that is, to consider the validity of the minimax theorem as central. The difference with Borel may be considered even more satisfactorily vis-à-vis the different way of interpreting the use of mixed strategies.

The mixed strategy interpretations that have actually been presented can be reduced to three categories (Dell'Aglio 1995): a *frequentist* category, that describes mixed strategies as the player's mean behaviour with respect to a large number of plays; a *psychological* interpretation that aims at describing the player's psychological behaviour and thus considers the assignment of a given probability to a strategy as the quantitative representation of the player's choice process; a *rationalist* interpretation based on the consideration that, if a player plays regularly, his opponent can guess his intentions and therefore probabilization of the strategies is an effective way of concealing these intentions and thus satisfying the requirement of rationality.

There is no doubt that Borel propended strongly for the psychological interpretation, and von Neumann for the rationalist one.[45] In his 1928 paper the latter pointed out that, by choosing a mixed strategy, the player «is protected against his opponent»

(Neumann (von) 1928b, see English translation, p. 23). For example, if this choice, between the two strategies, entailed attributing the value $1/2$ to each, «neither (not even he himself) could predict whether the choice was in favour of one or the other» (Ibidem).

In conclusion, von Neumann's approach was aimed at a normative theory – that is, a theory that sets the conditions in which it is possible to obtain a game solution that is compatible with a "rational" behaviour by both players – whereas Borel followed a decidedly descriptive approach. Von Neumann's successful demonstration of the minimax theorem directed the theory towards the normative approach.

Many years later one of Borel's pupils, the French mathematician Maurice Fréchet, raised a controversy against von Neumann, claiming that Borel preceded him in founding modern mathematical game theory (Fréchet 1953a, b). Von Neumann's response clearly revealed the divergence between the two points of view and the importance he attributed to the axiomatic approach (Neumann (von) 1953a). After acknowledging that Borel had been the first to introduce the concept of strategic form and of pure and mixed strategies, von Neumann pointed out that Borel had failed to found a theory of games as he had not believed in the minimax theorem. «There could be no theory of games without that theorem», he claimed, going on to add: «I developed my ideas on the subject before I read his papers, whose negative conclusion on the decisive point (the "minimax theorem", which alone makes the concepts in question unambiguously useful) would have been primarily discouraging».[46]

The latter considerations lead us to the issue of why and how von Neumann decided to concern himself with game theory. So far we have seen a von Neumann who had measured himself with all the central themes of science of the time and that in all of them had shown himself to be capable of suggesting conclusive responses. It cannot be claimed that the theory of games was one of the principal issues of scientific opinion at the time, nor indeed was it ever at a later stage.[47] It may safely be said that von Neumann's decision to concern himself with the theory of games was indicative of a genuine interest unaffected by any outside pressure from the dominant scientific paradigms.

45) The frequentist interpretation was never considered by Borel; it was considered in only a few rare cases by von Neumann in his later works.

46) Neumann (von) 1953a, 124. Note how deep rooted the divergence was, as Borel did not consider the demonstration of the minimax theorem to be a crucial step. As Robert Leonard correctly points out, «his unwillingness to sacrifice the mystery and delight of games for an elegant but inapplicable mathematics» is quite apparent (Leonard 1992, 49). Indeed Borel pointed out in 1938 that also in the case of very simple games «the very task of writing the equations, for instance, without mentioning their solution, seems to be absolutely unfeasible» (Borel et al. 1938, 115).

47) Even in our times the theory of games is not considered by mathematicians as a branch of central importance. Indeed its teaching is principally carried out in economics faculties, where it is actually even looked upon with suspicion by numerous economists. Several authors (see Mirowski 2002 and relative references) have claimed to infer the centrality of game theory from the widespread observation that the Hilbertian view of mathematics was that of a gigantic "combinatorial game". This was obviously only a simple metaphor without any precise reference to the theory of games in the strict sense, so that the argument is completely unfounded.

It can easily be seen that two factors were involved in the formation of this interest. The first is represented by the influence of the Hungarian mathematical environment, as well as of course by Zermelo's work. The second by a general interest by von Neumann in a rational management of social problems.

As far as the first factor is concerned, there is strong evidence that von Neumann's favourite interlocutors on the topic of the theory of games were his Hungarian compatriots Dénes König and Lászlo Kalmár, depositaries, together with himself, of the logical-set-theoretic tradition of Gyula König. This is shown by the comments referring to collaboration with von Neumann made in articles they published in the review "Acta Scientiarum Mathematicorum" of Szeged in 1927 and 1928 (König, D. 1927, Kalmár 1928). The most significant contribution by Dénes König again referred to the game of chess and to collaboration with von Neumann: he demonstrated that the number of moves by means of which a player can force a win from a winning position is finite. Again many years after, König, in his book on the theory of graphs, explicitly and repeatedly referred to collaboration with von Neumann on these topics.[48] Clearly, these authors had adopted an axiomatic and abstract approach that was closer to von Neumann's views.

We have seen how the sophisticated Budapest environment in which von Neumann lived and in particular his family environment encouraged the idea that social and economic phenomena could be treated and managed using rigorous scientific methods. As has been said, there was a pre-existing tendency to consider a general idea of "game" capable of including other forms of interaction among subjects and not only limited to parlour games, although there had been no development worthy of note at the scientific and mathematical level. The fact cannot be considered of marginal importance that, in his 1928 article, von Neumann – of whose interest in the socio-economic sciences no previous trace can be found – pointed to the analogy between game axiomatics and microeconomic theory. Indeed he wrote that

> [. . .] *any event – given the external conditions and the participants in the situation (provided that the latter are acting on their own free will – may be regarded as a game of strategy if one looks at the effects it has on the participants* [. . .] *this is the principal problem of classical economics: how is the absolute selfish* homo œconomicus *going to act under given external circumstances (Neumann (von) 1928b, English translation, p. 1).*

This was no coincidence since, as his compatriot, Nicholas Kaldor, in a meeting that took place in 1927, bore witness, von Neumann expressed an interest in economics, being advised by him to read *Über Werte, Kapital und Rente* (1893) by Knuth Wicksell. Kaldor also narrates that von Neumann rapidly read the book and told him that the approach used in Léon Walras' equations of economic equilibrium was inappropriate as it could lead to negative prices.[49] Further witness is borne by the economists

48) König, D. 1936. See on p. 116, note 1 and on p. 216, note 1. Again many years later von Neumann was among the few who recalled the work of Dénes König (cf. Neumann (von), Morgenstern 1944).

49) See Dore, Chakravarty, Goodwin (eds.) 1989.

Axel and Earlene Leijonhufvud who recall the intellectual life of Berlin in the late 1920s and refer to a seminar organized by Leo Szilard in 1926 on the role of mathematics in other disciplines which was held by Jacob Marschak (Weintraub 1983, 13). This testimony coincides with that given by Marschak himself in a letter written in 1973 to the science philosopher Michael Polanyi.[50] The seminar was attended by, among others, Polanyi, Wigner and von Neumann. When von Neumann saw the presentation of the equations governing supply and demand he intervened vehemently, claiming that it was necessary to replace the equations with inequations as the purchaser would at best have offered a given price and the seller would have demanded at least a given price.

It is therefore quite evident that the 1928 work on game parlour games was accompanied by a maturation of what would become a *Leitmotiv* in von Neumann's thinking: his interest in economic topics, the idea that the mathematization of economics must follow a completely new and original path compared with that proposed by the methods of classical analysis, the idea that the theory of games could represent the basic nucleus of a new mathematics which could be used to describe social processes. Since this project was completed in the 1940s, in the book written in collaboration with Oskar Morgenstern, *Theory of games and economic behaviour*, we shall postpone the description of his research in this field to a later chapter. However, it is important to note here that already in 1932 von Neumann presented at a seminar at Princeton a second work that represented a direct development of the preceding one. This link could escape notice, because he published this second contribution only in 1937 (Neumann (von) 1937).

This second work contains a mathematical model of production and aims at determining its equilibrium. What is impressive is the fact that the mathematical approach adopted represents a natural development of the approach that he had chosen in his 1928 paper on parlour games: the problem of equilibrium is reduced to a minimax problem and fixed point topological methods are used in its solution. However, since the "functions" involved prove to be multivalued correspondences, von Neumann generalizes Brouwer's theorem to take this circumstance into account. It is interesting to note that von Neumann himself later presented this result as a "generalization" of the minimax theorem contained in the 1928 work (see Neumann (von) 1953a).

Von Neumann's research on parlour games and production in this period is particularly significant, for two main reasons.

Firstly, it reveals a fundamental aspect of his thinking, namely his conviction that mathematics could escape an exclusive relationship with physical sciences and address all kinds of problems, including the rational treatment of social problems, particularly of an economic nature. There clearly emerges a very strong faith in the universal value of mathematics, a true panmathematical view of the real world.

Secondly, this early research shows his astonishing ability to work out radically new tools compared with those of classical mathematics for the purpose of mathema-

50) Cit. in Mirowski 2002, 102.

tizing any kind of situation and, in particular his early interest in combinatorial and finite schemata. Commenting on von Neumann's scientific personality, Jean Dieudonné claimed that

> *his genius lay in analysis and combinatorics, the latter being understood in a very wide sense, including the uncommon ability to organize and axiomatize complex situations that a priori do not seem amenable to mathematical treatment, as in quantum mechanics and the theory of games* (Dieudonné 1976, 89).

During the 1930s, von Neumann concentrated mainly on the developments of functional analysis. The ability described by Dieudonné, and which had already been outstandingly displayed during the Göttingen period, was later to be expressed in full. As his colleague Ulam asserted, «the influence of his work on mathematical logic later emerged in his discoveries concerning projects for calculating machine logic» (Ulam 1958, 14).

In this early phase of his researcher's career, von Neumann enjoyed a brilliant scientific success. His characteristic scientific outlook and his multifarious interests already stood out clearly. His mathematician's and scientist's personality was formed in those years: it was to be expressed to the full in development of the various research topics of his intense scientific activity in the United States. The cultural legacy of John von Neumann to contemporary thought was the fruit of a maturing scientific outlook that we shall describe in the following chapter.

Chapter 3

A Mathematician Between Past and Future

3.1 Continuity and evolution in von Neumann's thought

It may seem odd, at this stage, well before the end of this book, to find a chapter summing up von Neumann's scientific thought. To tell the truth, this chapter will in no way exhaust the analysis of the scientific conceptions of our protagonist. On the contrary, this analysis will represent the main thread running through the rest of the book. However, precisely at this point, that is, on the eve of a crucial passage in his life, namely his move to the United States, it seems necessary to scrutinize the ideas and views with which he prepared to make this transition. This will allow us to examine in greater depth and understand more fully the significance of all the subsequent developments.

Our choice could be criticized if it could be demonstrated that a gap or significant turning point occurred in his thought after his move to the United States. In such a case we could not hope for more than to present a summary of his previous views and then to show their diversity and discontinuity with the later ones.

Indeed several attempts have been made to identify points of discontinuity and even of rupture in his thought and his scientific work (especially in Mirowski 2002). In our view these are neither convincing nor well grounded. Of course, it is quite easy to identify several phases in his life marking changes in both his ideas and practice, and it is easy to make a list of them. We have seen how his contact with the Göttingen environment and with Hilbert was crucial to the formation of the young scientist, together with the multiple relations he entertained with the extremely rich European science of the late 1920s and early 1930s. The crisis in the Hilbertian formalist program certainly represented one of the most dramatic and influential events

regarding the evolution of von Neumann's ideas. Moreover, we shall also see the novelty represented by his introduction into the Princeton environment, and it is undeniable – this is without doubt the most important aspect – that all the developments that increased his fame took place after the end of the 1930s: his interest in numerical calculus, the construction of digital computers, the numerous applications of mathematical modeling to many branches of science and technology (military and industrial problems, meteorology, game theory, to mention but a few) and lastly automata theory. No precedents to these developments can be found in his "European" scientific work, so that they may be considered as something essentially new.

And yet, on close examination, all these aspects and novelties appear less as discontinuities, than as evolutions and transitions, as innovative as you like, but always systematically dominated by a personality founded on a very strong scientific view which surprisingly seems to have been formed and consolidated starting in the late 1920s. In Chapter 4 we shall see how America was the ideal soil which allowed the prodigious blossoming of a large number of new and highly original activities, the ideal terrain for the full development both of his scientific outlook and his practical tendencies. However, although fertile, the soil could not create the plant which, to pursue the metaphor, was much than a seed or a young shoot: von Neumann was a young scholar whose views and aims were already fully formed. In general, when we examine his intellectual itinerary, his contribution and legacy seem to far outweigh what he received or the influences he was subjected to. Nor would we expect anything different for a scientist who had had such a profound influence, like very few others, on twentieth century science and technology.

Attempting to identify the unitary core of von Neumann's thought takes us back to the years of his early youth, in which there already appears to exist a vision of extraordinary power and evidence that we could define as *a panmathematical world view.* Let us explain what is meant by this. "Panmathematicism" is not the classical Galilean idea according to which the physical world has been written (by God) in mathematical language, leaving it as a task for the scientist to decipher this writing in order to discover the essence of reality and the laws established (by God) and directing its course. This is both much less and much more at the same time. Much less because at no time is it possible to perceive in him any metaphysical, ontological conception of the role of mathematics, as is the case for Galileo. For von Neumann, mathematics is a language: the privileged language of logic, not the language of reality: it is the most effective *tool* for analyzing reality, not the essence of reality itself. But what is important is its efficacy: even when understanding of the regularities that apparently govern nature seems to be blocked by insuperable obstacles, it is essential that these obstacles be expressed in mathematical terms. Moreover, this (non-ontological) panmathematicism is more pervasive than the Galilean view, as it is not restricted to the physical world but projects its constructs onto all other aspects of reality: in particular, onto the living world and the world of social relations. The idea that mathematics can extend its field of action to embrace phenomena other than physical ones had been championed by a number of scholars, and in particular by members of the Vienna Circle, although it still mustered little support in the scientific

world in the years between the two world wars (Israel 1996a, 2002, 2004b). Nevertheless, this idea emerges already clear in von Neumann's work on game theory and on mathematical economics – published in two papers that we have considered in the final section of Chapter 1. Moreover, the modelling approach followed in these works – which we will have the occasion to illustrate shortly – represents another original and fully formed characteristic of his thought which already distinguishes it from Hilbertian outlook, which was still based on a reductionist view (Israel 1996a, 2004a).

Von Neumann's almost unbounded faith in mathematics, his tendency to analyze every aspect of the real world in logico-mathematical terms, represents the strong nucleus of his thought and is the key he used to overcome even the critical stages of his scientific itinerary, such as the crisis of the Hilbertian programme. There is no doubt that this event, through the revelation represented by Gödel's theorem, was one of the most dramatic shifts in von Neumann's intellectual itinerary, perhaps the only one that could be used to explain a discontinuity. Indeed, on this occasion, he changed his mind substantially. However, the crux of the matter is that, although he was compelled to give up a research program in which he had believed, he felt in no way compelled to give up his belief in the axiomatic method and thus in the universal value of abstract reasoning methods. Quite the contrary. Confidence in the axiomatic method as the only way out of the crisis was boosted by this event. For this reason, it is absolutely incorrect to speak of a gap in von Neumann's thought.[51]

A similar argument may be advanced for what is sometimes considered as the other great turning point, namely, his involvement in applied mathematical research in the military field and in numerical analysis, computers and automata. It is conceptually incorrect to oppose an initial phase mainly dedicated to "abstract", "pure" research to a second, more "concrete" and "empirical" phase.[52] There is no doubt that, towards the end of the 1930s, and above all as a consequence of his engagement in the military sector, von Neumann's concern shifted substantially towards applied science and technical-practical issues. William Aspray has traced interesting attesta-

51) As already pointed out (cf. note 38) this is an error made by Philip Mirowski (in Mirowski 2002 and 1992), probably because of the confusion between the Hilbertian formalist program and axiomatics: the latter is not a program but a concrete mathematical research practice rooted in the nineteenth century that Mirowski neglects as such and that it makes no sense to define as "Hilbert's program". This confusion led Mirowski to make bizarre statements such as the following one: «von Neumann's early work on game theory was prompted by questions thrown up by Hilbert's program on the formalization of mathematics» (Mirowski 2002, 108). It is almost superfluous to say that a demonstration of the self-consistency of mathematics has nothing to do with game theory.

52) Mirowski also falls into this error when he interprets von Neumann's transition towards a predominant interest in applications as an epistemological break in his thought (Mirowski 2002). In general, Mirowski's book consists of a vast and extremely useful collection of materials and documents, as well as of assessments – some of which we share, such as the contrast he establishes between von Neumann's game theory and Nash's. But its main shortcoming is that it is not a history book. What Mirowski proposes is instead a *rational reconstruction* of history in order to show the truth of a preset thesis. The key to this rational reconstruction is the concept of "cyborg science", which is only vaguely defined, artificial and totally unsuitable for framing the historical developments examined, while it clearly corresponds to the author's intention to criticize certain trends in twentieth century economics. As every rational reconstruction it has almost nothing to do with reality and only the author's ideological constructs emerge.

tions of von Neumann himself, in which he speaks about a different, "impurer" man. In a letter of May 1943 from England he wrote:

> *I think I see clearly that the best course for me at present is to concentrate on Ordnance work, and the Gas Dynamical matters connected therewith. I think that I have learned here a good deal of experimental physics, particularly of the Gas Dynamical variety, and that I shall return a better and impurer man.*[53]

Moreover, he stated in an address of 1955:

> *... it was through military science that I was introduced to the applied science. Before this I was, apart from some lesser infidelities, essentially a pure mathematician, or at least a very pure theoretician. Whatever else may have happened in the meantime, I have certainly succeeded in losing my purity.*[54]

However, this testimony tells us about a change in scientific *practice* and in the *object* of the research, but nothing about the *way*, the *method*, the *conceptual approach* with which he tackled new research fields. Nothing here authorizes us to infer that the attraction felt for new directly applied research themes was accompanied by a neglect of an axiomatic-abstract approach in favour of an empiricist approach. In actual fact, things are quite the opposite. As we shall see later, the originality of his approach – and the basis of its efficacy – consists precisely in having proposed the abandonment of an empiricist approach in technical and operating problems; and his great contribution – which extended far beyond the generic defense of theoretical science as the basis of success in applications fields – was precisely to have made available new theoretical tools for dealing with problems in a wide range of sectors. Whether in the field of computers, automata or weather forecasting, he suggested leaving aside the classical "practical" and "concrete" views that had hitherto dominated – and hindered! – development in these sectors and instead to start from a logical-formal formulation, that is, from abstract mathematical models. At the same time as he waxed enthusiastic over the problem of automatic calculus, he did not acquiesce to the approach hitherto adopted which consisted of improvements obtained by tinkering with the existing machines, but placed the power of the axiomatic approach at the centre of focus: it was first necessary to formulate the computer's logic in abstract terms before concerning oneself with the construction of a machine that implements this logic. Likewise, the equations of the dynamics of the atmosphere come first and are followed by their application to concrete forecasting processes. And so on.

---

53) Neumann (von) to Veblen, May 21 1943, O. Veblen Papers, Library of Congress, quoted in Aspray 1990, 27 ff.

54) These words quoted by William Aspray from the JNLC are part of a testimonial for Robert Kent, a researcher in the Army Ballistics Research Laboratory in Aberdeen (Maryland) (Aspray 1990, 26; a joint paper is Neumann (von), Kent 1940, and on von Neumann's collaboration with the Aberdeen Laboratory see below, Chapter 5).

Of course – and we repeat this – the new context of his work in the United States allowed him to discover the possibility of applying mathematics concretely – very concretely. However, this represented above all an outright triumph of the abstract axiomatic method in mathematics. Indeed, in this way, it was shown that this method effectively underlay a theoretical approach also in the technical and practical field: what military art, industrial practice, weather forecasting and calculating machines had once been were now becoming fields of applied *science* which adopted the language and conceptions of mathematics. For von Neumann this involved the attainment of an old objective, and certainly the "loss of his purity" did not mean that he was following a direction opposite to the preceding one.

It is this fundamental continuity that gives meaning to the attempt to outline the features of von Neumann's scientific conception before setting about describing the most extraordinary and innovative phase of his life, which took place in America.

3.2 Axiomatics and the twentieth century renewal of mathematical practice

As we have seen, in the early twentieth century, the Göttingen school set about making a profound transformation of mathematics. The axiomatic approach strongly encouraged by Hilbert was not a simple contribution to the debate on the foundations of mathematics: this contribution, as represented by the "formalist" programme, although important, was certainly ephemeral. By far the most influential aspect was the process of radical reorganization of mathematical practice stimulated by the axiomatic approach and of which the Göttingen school was the most conscious interpreter. One of the main products of this approach was the development of "modern" or "abstract" algebra. The efforts made to reconstruct and develop nineteenth century algebraic theories – such as the theory of groups or the theory of fields, both arising out of the problem of the solution of algebraic equations, whose most brilliant expression was embodied in Galois' theory – and the introduction of new abstract structures, radically modified both the conception and the role of algebra. In 1931 the Dutch mathematician Bartel Leendert van der Waerden, who had worked at Göttingen under the direction of Emmy Noether, published a text book, *Moderne Algebra*, which set forth a systematic treatment of the contributions of thirty years of research in algebra, also with a view to encouraging its inclusion in university syllabuses (Waerden (van der) 1931). This book was a milestone that marked the consolidation of this new branch of mathematics, the decline of the intuitive geometric approach and with it, the end of the central position occupied by geometry in classical mathematics.

A further development of this tendency was the rise in France, during the period of World War II, of the so-called "Bourbaki" movement.[55]French mathematics in the 1920s and 1930s had not followed the axiomatic trend and had remained attached to

55) On the Bourbaki movement, see: Israel 1977, 1978; Corry 1992, 1996, 1997; Beaulieu 1990; Aubin 1997; Mashaal 2006.

the idea of centrality of the theory of functions and the relationship between analysis and physical mathematics. It had consequently completely lost the leading role it had played in the nineteenth century and had indeed suffered a severe decline in the context of international research.[56] One important attempt to re-establish the links between French mathematics and new research trends was the Paris Seminar directed by the French mathematician Jacques Hadamard. Although not enough to stem the decline, this initiative encouraged a group of young and brilliant mathematicians to group together under the pseudonym "Nicolas Bourbaki" (this pseudonym was apparently taken from the name of one of Napoleon's generals, Nicolas Bourbaki).

The group's primary aim was to disseminate the new axiomatic and algebra-based ideas. To this end, work was started on publication of a monumental treatise entitled *Eléments de mathématique*. The purpose of this book, the centre of elaboration of which was the seminar cycles that gathered together the members of the group, was to reconstruct the entire edifice of mathematics on axiomatic bases, putting together the innovative work of mathematicians in the past decades, and to provide a twentieth century equivalent of Euclid's *Elements*. The project drew its inspiration from the Hilbertian model of the *Foundations of geometry*, although it went much further. The "bourbakists" actually did not stop short at axiomatization of individual theories or, to use their terminology, to the consideration of "univalent" axiomatics, i.e., applicable to a single theory, as in the case of the Hilbertian axiomatization of Euclidean geometry: their objective was, through the process of axiomatization to invest the whole edifice of mathematics, starting from the foundations (set theory). A crucial tool of this process was the concept of *mathematical structure*. According to the "bourbakists", the whole of mathematics could be reduced to the interweaving of three fundamental axiomatic structures: *algebraic* structures, *order* structures and *topological* structures. These ideas reached their height in the 1950s and 1960s, although they had already been illustrated in the "bourbakist" manifesto, "The architecture of mathematics" (Bourbaki 1948).

These profound changes in the discipline had other important consequences. The abstract axiomatic approach led to a standardization of the methods, content and language of mathematical research that encouraged the spread of an internationalistic spirit, which recovered the ancient universalistic spirit of mathematics through gradual dissolution of the national styles that had dominated nineteenth century mathematical "schools".[57] The central role of the national mathematical communities typ-

56) One joint cause of this decline was also the massive death rate of young French scientists (especially mathematicians) in the front line during World War I.

57) See Parshall 1996, which summarizes the numerous national cases examined in recent years. The historical analysis of the rise and fall of the national schools has yielded an improved knowledge of the relationship between mathematics and culture in the nineteenth century. Nevertheless it may be considered a transient phase in a historical evolution dominated by the universal and transnational nature of the discipline that is now widespread in the world of mathematics; it was not born at that time, as Parshall claims, but was reconstructed in the early twentieth century thanks to the link up among national experiences (as is actually described correctly in the article). The persistence of the universalistic ideal is quite clear in one of the emblematic events of this process, that of the Palermo Mathematical Club, the Circolo matematico di Palermo (Brigaglia, Masotto 1982).

ical of the later nineteenth century was replaced by a tendency to encourage contacts and to disseminate information at the international level. Even though the aftermath of World War I hindered this trend, it nevertheless became an unstoppable process. Even the nationalism that emerged between the two wars paradoxically ended up by favouring this process: the disintegration of the Göttingen school under nazism spread throughout the world, through the migration of practically all its exponents, the knowledge of a research practice that was to become a generalized model.

Another important consequence was a transformation of the relationship between mathematics and the other sciences, and of the relationship between pure and applied mathematics. At the beginning of the century, the crises affecting a number of scientific disciplines, and which shook their very foundations, led to a breakdown in the unitary fabric of science; a fragmentation and a specialization of research began to spread through the scientific community, partly as a result of the extraordinary development of specific new branches and of an astonishing increase in the number of professional scientists. The increasing complexity and scattering of scientific activity did not prevent a profound need for the unification of research to continue to be felt in many scientific circles, not only in the natural sciences but also in the social sciences. The debates within the Vienna Circle, which were to have a strong influence on science scholars, and in particular on many German researchers in the field of quantum mechanics, were focused intensely on this topic. One of the main initiatives of the Circle was the organization of an international movement for the unity of science, in which researchers working in a wide range of different fields participated, for instance the Polish school of logic, the Italian review *Scientia*, the American pragmatist school, the newborn analytical philosophy, the school of logical conventionalism and of course that of neopositivism or logical positivism, directly inspired by the Circle. This movement promoted a number of congresses as well as the project of an *International encyclopaedia of unified science*, which was continued when the war ended.

What was the role played by mathematics in this context? The traditional view of the relationship between mathematics and reality based on the model of Newtonian mechanics was affected by an irreversible crisis. This view was based on the principle of *mechanical reductionism*, that is, on the idea that every phenomenon can be "reduced" to a description given in terms of motion processes. The scientific analysis of each complex of phenomena was guided by the method of *mechanical analogy*, that is, by the representation (or restating) of this set of phenomena as a mechanical scheme; and, in mathematical terms, it was based on the use of classical mathematical analysis (differential and integral calculus and the theory of differential equations). In the 1920s, Vito Volterra made a further attempt to apply this reductionist scheme to the mathematical study of a complex of biological phenomena involved in population dynamics (Israel, Millán Gasca 2002, Israel 1988, 1991). However, the general trend was towards replacing this approach with a more flexible one of *mathematical analogy* based on the attempt to find abstract, not necessarily mechanical, mathematical descriptions that could be applied to even widely differing situations belonging to domains as far apart as physics, chemistry, biology or economics. This kind of description – mathematical models – made it possible to unify differing phenomena on

the plane of a purely linguistic-formal analogy provided by the mathematical scheme. According to the expression used in the bourbakist manifesto, it was a question of constructing «mathematical models, abstract schemata of possible reality» (Bourbaki 1948, 46). Confidence in mathematics as a tool for investigating reality began to take on decidedly idealistic traits:

> *In the axiomatic conception, mathematics actually behaves as a reservoir of abstract* forms *– the mathematical structures; and it is true that – without knowing exactly why – certain aspects of experimental reality are shaped into several of these forms, as though by a kind of preadaptation. It is undeniable, of course, that the majority of these forms originally had a well-defined intuitive content; however, it is precisely by deliberately emptying them of this content that it has been possible to realize all their potential effectiveness, and enable them to be interpreted in new ways and to completely fulfill their elaborating role.*[58]

Clearly this approach abandoned the classical conception of the relationship between experiment and mathematical description, drastically reducing the initially central role of the empirical verification of mathematical theories. Furthermore, for researchers drawing inspiration from this conception, empirical problems ceased to be the main source of identification of mathematical problems, thus introducing a distinction inside mathematics between "pure" mathematics, guided by completely internal criteria, and "applied" mathematics, based on problems raised by technology or by practical activities (such as finance, management and so on).

It is worth emphasizing another aspect of the intellectual horizon of the philosophical-scientific discussions at the beginning of the twentieth century which will be treated in greater detail in the following section. The discussion of the foundations of mathematics seemed to have definitively been closed after the proof of Gödel's theorem, which demonstrated the impossibility of further pursuing the formalist project. This theorem abruptly extinguished the interest of many mathematicians, including von Neumann, in the question of foundations. In the 1940s and 1950s, the philosophy of mathematics did not identify any new problems with which to replace the question of foundations, so that the majority of mathematicians and philosophers continued to identify the philosophy of mathematics with the theory of the foundations of mathematics, or with mathematical logic. As a consequence of this state of affairs and of a pragmatic interpretation of the axiomatic method – to which, as we have seen, a decisive contribution was made by von Neumann himself – the majority of mathematicians lost interest in the philosophical aspects of the discipline. Mathematical logic, on the other hand, developed topics arising out of this context and became an actual specialist discipline, mid-way between philosophy and mathematics, as well as engaging in an intense interaction with computer science. Lastly, in the philosophical field, the echo of the discussions of logic and the foundations of mathematics

58) Bourbaki 1948, 46–47. On the origins of mathematical modelling and its epistemological features see Israel 1996a and 2002.

helped to bring into the foreground, starting from the Vienna Circle contributions and Wittgenstein's work, the problem of the analysis of language, which led to the development of analytical philosophy. This mathematics-based philosophy enjoyed an extraordinary expansion in the second half of the 20th century. It belongs to the postmodern school of thought, which held that several of the great issues of classical philosophy were insignificant in that they were "metaphysical" and thus set them aside. The emphasis laid by many mathematicians on mathematics as a language and as a formal and analytical tool appeared to be consistent with this approach.

3.3 Von Neumann's conception of mathematics

The writings in which von Neumann expresses his conception of mathematics all date to the postwar period. However, the influence of his previous cultural background is revealed by the vivid and intense impressions he communicated of the years leading up to World War II. In 1947, in a collection of essays entitled *The works of the mind*, he published a now famous discussion concerning the nature of intellectual endeavour in the field of mathematics, under the title of "The mathematician". Developing his ideas concerning rigor in mathematics in relation to the problem of the role of mathematics as a knowledge tool, he gave a detailed description of the quarrel over the foundations of mathematics, which he concluded with the following comment, which clearly illustrates how he felt at the time:

> *I have told the story of this controversy in such detail, because I think that it constitutes the best caution against taking the immovable rigor of mathematics for granted. This happened in our lifetime, and I know myself how humiliatingly easily my own views regarding the absolute mathematical truth changed during this episode, and how they changed three times in succession!* (Neumann (von), 1947a, in JNCW, vol. 1, 6)

A similar comment appears in "The role of mathematics in science and society", which forms the text of an address delivered at the 4th conference of the association of Princeton Graduate Alumni several years later, published in 1954.[59] In both these documents the central issue is whether mathematics has or has not an empirical nature, its relationships with the natural sciences, and more in general, with any science that interprets experience at a higher level than the purely descriptive level. We shall discuss von Neumann's point of view on these topics in the following, but first it is necessary to say something about the meaning and importance of the first great contributions that von Neumann made to mathematics and science, that is, in the course of his European career.

We have repeatedly emphasized that the early and intense interest shown by the young von Neumann in topics related to mathematical logic strongly influenced the way he was later to approach other research issues. His first important contribution

59) Neumann (von) 1954a. We shall come back to this short essay in the next section.

to the axiomatization of set theory was related to his unshakeable confidence in the possibility of approaching a wide range of topics in axiomatic and abstract terms; this confidence was reinforced by the brilliant contribution he made to the foundations of quantum mechanics and the no less brilliant proof he gave of the minimax theorem in game theory, which was to open up for researchers exciting possible applications to a wide range of problems extending well beyond the classical field of exact sciences.

This confidence was so solid that it was not affected by the negative parabola of Hilbert's formalistic programme and of the more general project of a logical refounding of mathematics, and by the disappointment that von Neumann felt over this failure. Shrugging off this phase, he maintained a firm anti-intuitionist position and became the champion of that vast majority of mathematicians who succeeded in overcoming the crisis of those projects by vehemently confirming their allegiance to a research practice consolidated by a long tradition and many successes. It is interesting to follow his thoughts on this subject.

> *In the late nineteenth and the early twentieth centuries a new branch of abstract mathematics, G. Cantor's theory of sets, led into difficulties. That is, certain reasonings led to contradictions; and, while these reasonings were not the central and "useful" part of set theory, and always easy to spot by certain formal criteria, it was nevertheless not clear why they should be deemed less set-theoretical than the "successful" parts of the theory. Aside from the* ex post *insight that they actually led into disaster, it was not clear what* a priori *motivation, what consistent philosophy of the situation, would permit one to segregate them from those parts of set theory which one wanted to save. A closer study of the merits of the case, undertaken mainly by Russell and Weyl, and concluded by Brouwer, showed that the way in which not only set theory but also most of modern mathematics used the concepts of "general validity" and of "existence" was philosophically objectionable. A system of mathematics which was free of these undesirable traits, "intuitionism", was developed by Brouwer. In this system the difficulties and contradictions of set theory did not arise. However, a good fifty per cent of modern mathematics, in its most vital – and up to then unquestioned – parts, especially in analysis, were also affected by this "purge": they either became invalid or had to be justified by very complicated subsidiary considerations. And in this latter process one usually lost appreciably in generality of validity and elegance of deduction. Nevertheless, Brouwer and Weyl considered it necessary that the concept of mathematical rigor be revised according to these ideas.* (Neumann (von) 1947a, in JNWC, vol. 1, 5)

According to von Neumann, the majority of mathematicians continued to violate these new standards of rigour, and did their mathematics following the old practice, probably hoping that someone would sooner or later find an answer to the intuitionist criticism. Here is the description von Neumann gives us of the situation caused by Gödel's theorem:

> *The main hope of justification of classical mathematics – in the sense of Hilbert or of Brouwer and Weyl – being gone, most mathematicians decided to use that system anyway. After all, classical mathematics was producing results which were both elegant and useful, and, even though one could never again be absolutely certain of its reliability, it stood on at least as sound a foundation as, for example, the existence of the electron. Hence, if one was willing to accept the sciences one might as well accept the classical system of mathematics. Such views turned out to be acceptable even to some of the original protagonists of the intuitionistic system. At present the controversy about the "foundations" is certainly not closed, but it seems most unlikely that the classical system should be abandoned by any but a small minority.* (Neumann (von) 1947a, in JNWC, vol. 1, 6)

These words describe the way in which the crisis of the Hilbertian program caused by Gödel's theorem was "metabolized" by the mathematical community and the prevailing feeling that spread, establishing a new form of confidence in the practice of the discipline. Confirmation is forthcoming in the words of a mathematician with a highly different profile from that of von Neumann, even though he shared the same confidence in axiomatics: Jean Dieudonné, one of the leaders of the "bourbakist" movement, an emblematic representative of the "pure mathematician" of the mid twentieth century. This is what Dieudonné wrote in 1951, describing feelings that were practically identical to that described by von Neumann:

> *For us, all the axiomatic theories are placed on the same plane: therefore we deem unacceptable the intuitionists' objection that the rules of logic and of set theory could only be applied to mathematical entities to which they have attributed a privileged "concrete" character* (*such as whole numbers*)*, and not to those considered by an axiomatic theory whatever.*
>
> *The modern mathematician thus feels his conscience is perfectly clear and is no longer concerned with all the pseudo-problems with which his predecessors were obsessed. Classical logic and classical set theory forming the basis of his language have long since been systematized so as to respond to all his needs, completely exorcising the "paradoxes" that terrified Cantor's contemporaries. The actual monster Contradiction, so persistently brandished by Poincaré against axiomatics, lost its fearful aspect: for us, to observe that a theory, whose axioms are A, B and C, is contradictory, simply means demonstrating the "non C" proposition in the theory whose axioms are A and B; from this point of view, it may be claimed without paradox that, for the mathematician, it is more interesting to prove* mathematically *that a theory* is *contradictory* (*which is a theorem*)*, than to demonstrate* metamathematically *that em it is not* (*which simply means that there is no hope of ever demonstrating* mathematically *that the theory is contradictory*)*. All the issues such as the*

> *non-contradiction of theories, the* Entscheidungsproblem*, and in general everything related to proof theory now belongs to a science that is totally detached from Mathematics, Metamathematics; this new discipline is constantly developing and has already yielded numerous new and highly interesting results; however, it is perfectly legitimate for the mathematician to ignore them completely without this in any way impeding him in his research.* [...]
>
> *The great critical reappraisal that followed the 1900 crisis has established relatively solid bases that allow us to feel quite immune from this danger* [*the danger that philosophers can defeat mathematicians reasoning from the inside*]. (Dieudonné 1951, 51–52)

It has been said that this pragmatic view, in the field of both physics and mathematics, was actually based on a "metaphysical" conviction of the "validity" (in a somewhat ill-defined meaning of the term) of these knowledge systems: a view and a conviction that were reinforced by the systematic use of the axiomatic approach. Despite the failure of Hilbert's formalistic programme, his source – the idea of axiomatization – had proved to be the key to banish the "monsters" standing in the way of the triumphant march of the "exact" sciences (at least on a pragmatical plane). Von Neumann had made a decisive contribution to driving these monsters away from the basic ground of mathematics: the axiomatization of set theory had represented a fundamental step in that process of "systematization" that was capable of "responding to all the mathematician's needs, totally exorcising the 'paradoxes' that terrified Cantor's contemporaries", to use Dieudonné's words. But von Neumann had been the author of a similar operation in quantum mechanics, which allowed the continuity with classical mechanics to be preserved. He had made a careful and thorough study of the emergence of chance and indeterminism and the probabilistic interpretation of quantum mechanics, and his work in refounding this theory had allowed him to introduce some control over these aspects, which was rigidly exercised by the axiomatic theoretical presentation. This result represented the acme of an effort to retain the classical image of science as the depository of the truth of the physical world, capable of expressing this truth by means of deterministic laws.

3.4 The language of mathematics and determinism

A more precise idea of the operation carried out by von Neumann, which formed the basis of the immense prestige and influence he enjoyed in the twentieth century scientific world, may be obtained by considering that the impact of his work on the mathematical foundations of quantum mechanics extended beyond the confines of quantum theory and actually had the effect of plugging the gaps in classical mechanics, which seemed irreparable at the beginning of the century. His role was to restore confidence in the idea that determinism (that is, the principle according to which the state of a system at any given instant in a univocal and rigidly causal way "determines" its evo-

lution in time) still had an important function in scientific epistemology; as a result, for a long time, debate on this topic attenuated. Only in more recent times – starting in the 1960s – was there further discussion on the validity of determinism, after the discovery of "chaotic" processes which, although deterministic, are unpredictable – or at least the cost of their predictability increases exponentially.

We shall not go into the conceptual confusion as a result of which an absolutely disproportionate anti-deterministic importance was attributed to the discovery of deterministic chaos, almost as though it represented the destruction of causalism from the inside (Israel 2005). We shall merely point out that, in actual fact, many of these issues – which have aroused an attention that is not always completely lucid, not only in the scientific world, but also in philosophical and intellectual circles in general – had been identified at the end of the nineteenth century by eminent scientists precisely in the context of classical mechanics. For example, the concept of irreversibility and the implications of the second law of thermodynamics were pointed out by Ludwig Boltzmann and Max Planck, but these issues remained within the confines of physics until Ilya Prigogine brought them to the attention of the entire scientific community.[60] Poincaré himself had already investigated the problem of dynamic instability within one of the most classical themes of Newtonian mechanics, the three body problem (Poincaré 1892–99): he had come up against the same phenomenon – the "chaos" that occurs in nonlinear deterministic dynamical systems – which today is studied in "complex" systems, not only in physics but also in biomathematics and economics. No less important was Hadamard's discovery of certain "chaotic" behaviours of geodetic curves on surfaces having a negative curvature (Hadamard 1898).

All these questions and their accompanying mathematical problems had their champions in the early twentieth century, in narrowly defined disciplinary areas. Nevertheless, it may legitimately be claimed that von Neumann's intervention shifted these issues away from the central area of mathematical and theoretical physics research. His intervention in the direction of research was closely linked to an important change of approach at the level of method and mathematical techniques, for which he was one of those mainly responsible. The *mathematics of time* born from the Newtonian revolution and which developed in symbiosis with differential calculus gave way to a *static and atemporal mathematics*. In the latter the emphasis is laid on algebraic and topological structures, and the techniques of functional analysis, measure theory and convex analysis flourish, together with the use of fixed point theorems. Fewer differential equations and more inequalities, seems to be the watchword in the new mathematics.

In the first half of the twentieth century the philosophical issues raised by developments in classical analysis were thus avoided or incorporated into a more abstract mathematics. Out of the ashes of mechanistic reductionism and classical determinism there arose a neo-reductionist view represented by von Neumann's scientific paradigm, which we have already defined as *panmathematical* in the sense that its key idea was the central nature of mathematics viewed as a logical-deductive scheme, as

60) See for instance Prigogine 1962.

an abstract language of great unifying power. In his view, this idea resolved the old philosophical-scientific debate on determinism, causalism and finalism, which was practically dissolved, following von Neumann, by considering the problem from a rigorously mathematical point of view. In the 1954 Princeton lecture text cited earlier, he declared:

> *This great flexibility* [*of mathematics*], *to which I allude, involves things like this: In normal terminology it is considered a problem, which has occupied philosophers greatly in discussing an area, whether the laws which control this area are of a following nature. Each event determines the event immediately following upon it directly. This is the causal approach. Alternatively, these laws might be teleological, which means that a single event does not determine the next event, but that somehow the whole process must be viewed as a unity, subordinate to a general law so that the whole can only be understood as a whole. If I say that this has beset the philosophers I am understating. This has played a very great role, and is still playing a very great role, for instance in biology.*
>
> *Well, I don't say this is a bad question, or a meaningless question, but it is a great deal more subtle, at any rate, than it sounds; because a good deal of mathematical experience shows that unless you are awfully careful, the question has no meaning.* (Neumann (von) 1954a, in JNCW, vol. 6, 482)

He justifies this claim on the basis of an analysis of the two mathematically equivalent formulations of classical mechanics. The original Newtonian version – he explains – was strictly causal. In this formulation, the law of motion for a body is obtained by solving Newton's equation expressing the proportionality between the body's acceleration and the force acting on it ($f = ma$, where f is the force, a the acceleration and m the body's mass). Since acceleration may be interpreted as the second derivative of space versus time, Newton's equation is an ordinary differential equation. The solutions of such an equation, which give the trajectories of the body in motion, are characterized by the content of a theorem ("theorem of the existence and uniqueness of solutions") which states that the initial state of the body univocally determines its future (and past) mechanical evolution: this accounts for the purely causal nature of the Newtonian formulation.

In the eighteenth century a different approach was proposed based on the so-called "least action principle", formulated for the first time by the French physico-mathematician Pierre Louis Moreau de Maupertuis (Israel 1997). In this "variational" formulation, the motion of a mechanical system is governed by a teleological or finalistic principle: nature acts in pursuit of an end, which consists of saving a given quantity that Maupertuis identified as the "action" (integral of the product of the mass, velocity and the space travelled by the body). The trajectory actually followed by the body is such as to minimize the "action" and thus allows the objective of economy to be achieved. In this way, it seemed to be possible to reformulate mechanics on a non-causal basis by reinstating a qualitative and teleological view typical of Aristotelian

physics: motion is not governed by a blind law of determination but by Nature's "intention" to achieve an objective. Nevertheless, the mathematical analysis of the two formulations led, at the end of the nineteenth century, to the realization that they were totally equivalent, at least on a purely mathematical plane: the results obtained by solving Newton's equations or by minimizing the action are exactly the same, which shows that also the variational formulation follows a causal behavior.[61]

Without going into the history of the two conceptions, but merely observing the subsequent demonstration of the equivalence of the two formulations, von Neumann thus points out that the distinction between the two approaches exists solely at the level of a more superficial analysis, while, at the mathematical level – i.e., the one revealing the essence of things – it vanishes:

> *The first approach is strictly causal, working from point to point in time. The second is strictly teleological, and defines only the total history by virtue of certain optimal properties, not any part of it. Yet the two are strictly equivalent; the actual history for movements that you derive from one is precisely that which you find from the other; and the question as to whether mechanics is causal or teleological* (*which in any other field would be viewed as an important substantive question calling for a yes or no answer*) *is manifest nonsense in mechanics, because it depends purely on how you choose to write the equations. I'm not trying to be facetious about the importance of keeping teleological principles in mind when dealing with biology; but I think one hasn't started to understand the problem of their role in biology, until one realizes that in mechanics, if you are just a little bit clever mathematically, your problem disappears and becomes meaningless. And that it is perfectly possible that if one understood another area the same might happen.*
>
> *This is an insight which would probably never have been obtained without the pure mathematical trickery of transforming the equations of mechanics; it was purely mathematical skill and the flexibility characteristics of mathematical formulation and re-formulation, that produced this insight. It is not pure thinking at any abstract level, but a specifically mathematical procedure.* (Ibidem)

What is expressed here is an anti-metaphysical position in line with the positions of logical neopositivism, which has often been confused with an alleged contempt by von Neumann for philosophical topics, due to a logical mentality and a passion for mathematics that were believed to have led him to consider philosophical issues as unimportant. In this connection, Heims discusses the figure of von Neumann as the epitome of the technocratic myth which presents the scientist as the "new man" typical of the mid twentieth century.[62] There is no doubt that, during the second half

61) In this connection see Ernst Mach's discussion in Mach 1883.

62) See Heims 1980, 360 ff.

of the twentieth century, the scientific community was dominated by a kind of disdain for philosophical issues regarding scientific knowledge and the role of science in society and culture, but it is somewhat reckless to suggest von Neumann as the model for such attitudes or to claim that he encouraged them by his example. On the contrary, he held a much more comprehensive view. The above quotation reveals a strictly philosophical reductionist position, at the centre of which there persists a classical view of mechanics as the paradigm of a scientific discipline: mechanics is a model by virtue of its high degree of mathematization, which enables it to reveal the essence of phenomena, and if metaphysical concepts persist in biology, it is due to its low mathematical level. Yet in von Neumann's thought, the emphasis is placed, rather than on the mechanical scheme, on the abstract mathematical formulation, which allows the apparently non-causal factors to be reabsorbed, reducing them to the laws of logic. A similar situation occurs also in the relationship between causality and chance: here also the abstract mathematical formulation allows chance to be reduced to the laws of logic. This is what happened, he points out, with the theory of probability and with quantum mechanics, and here he is merely describing the sense of his work of axiomatization:

> *It turns out that the elementary processes – the processes involving elementary particles, the atoms or possibly sub-atomic particles – are, in spite of everything known previously, apparently not subject to laws like those of mechanics, because the laws of mechanics in their causal form tell you that if you know the state of the system now, you can tell exactly the state a short time afterwards, and by repeating this you can tell what it will be like at all times afterwards. It turns out that for the elementary processes it doesn't look as if it were this way. The best description one can give today, which may not be the ultimate* (*the ultimate one may even revert to the causal form, although most physicists don't think this is likely*) *but at any rate the best we can tell today, is that you do not have complete determination, and that the state of the system now does not determine at all what it will be immediately afterwards or later. Of course, a state now may be incompatible with some further assumptions about what it will be an hour later; or some of them may be extremely improbable. But many possibilities will still be left; and one might suspect that this is an idea which does not lend itself to description by precise mathematical means.* (Ibidem, 485–486)

It should be noted that, although in this quotation he is summarizing his point of view several years after his research in the field of quantum mechanics, he goes as far as to claim that "the best description one can be given *today*" is indeterministic, although this does not allow us to say it is "the definitive one": "the ultimate one may even revert to the causal form", even though this is considered unlikely by most people. Essentially he does not move far from the line of thought outlined one and a half centuries earlier by Pierre-Simon Laplace, who stated the substantial validity of the

causalist view, while declaring it unpursuable in practice in a number of cases, which led him to introduce the tool of probability calculus. The essence of von Neumann's thought is that there is no crisis in the principles of classical science, since this is guaranteed by mathematics. Mathematics can perform the fundamental task of bolstering confidence in the existence of an *order* in physical phenomena, even when it is impossible to implement a traditional determinist paradigm:

> *A system, like the one here referred to, is not causally predictable. You cannot calculate from its present state its state at the next moment. There is, however, something else which is causally predictable, namely the so-called wave-function. The evolution of the wave-function can be calculated from one moment to the next, but the effect of the wave-function on observed reality is only probability. That such a combination can at all be worked out, that it can decipher experience, and even be derived from experience, is something which again would have been completely impossible if the mathematical method had not existed. And again an enormous contribution of the mathematical method to the evolution of our real thinking is, that it has made such logical cycles possible, and has made them quite specific. It thus made it possible to do these things in complete reliability and with complete technical smoothness.* (Ibidem)

In order to complete what has been said about his acceptance of the (possibly transitory) role of the descriptions in somewhat random terms – in any case with an underlying implicit "strong determinism" – it is interesting to note an assertion he made in a more informal tone in a letter written in 1955 to the physicist George Gamow, who had formulated a theory of protein formation based on random processes:

> *I shudder at the thought that highly efficient purposive organizational elements, like the protein, should originate in a random process.*[63]

It is thus interesting to note how, although von Neumann gave a tremendous impulse to the "new mathematics" of the twentieth century, his mentality as a scientist and mathematician remained faithful to the classical conception. From this standpoint he was representative of a period of cultural transition. Although drawing inspiration from an abstract conception, he never subscribed to the Bourbaki idea of a radical distinction between pure mathematics and applied mathematics and of the intellectual superiority of the former over the latter. On the contrary, he always displayed a strong and direct interest in applications, and in this context he retained an approach aimed at recovering the value of classical reductionism. He held a conception of the relationship between mathematics and reality that was actually much closer to Hilbert's original view than to Bourbaki's, which represented the extreme evolution of the Hilbertian view. In this connection, mention must be made of Klein's statement (in a letter written to Wolfgang Pauli in 1921), with reference to research on

63) Quoted in Heims 1980, 154.

the foundations of the theory of general relativity, that Hilbert held «a fanatical belief in variational principles and the opinion that one can explain the essence of nature merely through mathematical reflections».[64] A similar statement could be made to apply also to von Neumann simply by replacing variational principles with logical-algebraic structures.

On the other hand, von Neumann's idea of mathematical model was much less classical and placed the accent on formal and analytical language, clearly distinguishing itself from a realist view of knowledge and from the belief that science can attain the "truth" of nature. In this connection, in 1955, in an article entitled "Method in the physical sciences", published in the book *The unity of knowledge*, he wrote

> [...] *the sciences do not try to explain, they hardly even try to interpret, they mainly make models. By a model is meant a mathematical construct which, with the addition of certain verbal interpretations, describes observed phenomena. The justification of such a mathematical construct is solely and precisely that it is expected to work – that is, correctly describe phenomena from a reasonably wide area. Furthermore, it must satisfy certain aesthetic criteria – that is, in relation to how much it describes, it must be rather simple.* (Neumann (von) 1955a, in JMCW, vol. 6, 492)

Also here we perceive the distance that separates von Neumann's panmathematicism from that of Galileo's views or even Einstein's, not to mention Newton's. For Galileo, mathematics is the essence of nature, which makes it possible to attain the "truth". For Newton, the task of natural philosophy is to trace back the causal chain leading from each phenomenon to the Prime Cause (that is, God). For von Neumann, the claim to explain and even interpret phenomena is excessive – it is too ambitious an objective that science has left behind it. Science makes models, of which mathematics is the language of choice, the most suitable and effective. The modernity of this view is quite apparent, because it fully interprets the modelling paradigm that had been established as early as the late 1920s. Indeed, von Neumann's early papers on game theory and in mathematical economics were among the first examples of *mathematical models* to appear in scientific literature and are as indicative of modern modelling as the relaxation oscillation model (and its many applications) formulated by the Dutch electrical and electronic engineer Balthazar Van der Pol and the model of the US statistician Alfred Lotka.[65]

However, von Neumann's view of scientific research praxis differs from the characteristics that modelling praxis would take on later, owing to the role that he continued to attribute to empirical verification: when he spoke of a "model that works", "that correctly describes the phenomena", he was thinking of an effective control dictated by the needs of concrete applications, with which he never lost touch. More explicitly, although asserting the autonomy of mathematics and accepting the existence

64) Quoted in Rowe 1989, 212.

65) Neumann (von) 1928, 1937 (it should not be overlooked that the formulation of the production model contained in the 1937 paper dates to the early 1930s); van der Pol, van der Mark 1928; Lotka 1925; see Israel 1993b, 1996a, 2002, 2004a.

of internal or "purely aesthetic" criteria that were able to guide research, he believed in the need to avoid breaking the link between mathematics and the real world. In his view, there is a tension inside mathematics between its internal, aesthetical, pure needs and the need for a relationship with empirical reality, which he described as follows:

> *I think that it is a relatively good approximation to truth – which is much too complicated to allow anything but approximations – that mathematical ideas originate in empirics, although the genealogy is sometimes long and obscure. But, once they are so conceived, the subject begins to live a peculiar life of its own and is better compared to a creative one, governed by almost entirely aesthetical motivations, than to anything else and, in particular, to an empirical science. There is, however, a further point which, I believe, needs stressing. As a mathematical discipline travels far from its empirical source, or still more, if it is a second and third generation only indirectly inspired by ideas coming from "reality", it is beset with very grave dangers. It becomes more and more purely aestheticizing, more and more purely* l'art pour l'art. *This need not be bad, if the field is surrounded by correlated subjects, which still have closer empirical connections, or if the discipline is under the influence of men with an exceptionally well-developed taste. But there is a grave danger that the subject will develop along the line of least resistance, that the stream, so far from its source, will separate into a multitude of insignificant branches, and that the discipline will become a disorganized mass of details and complexities.* (Neumann (von) 1947a, in JNCW, vol. 1, 9)

This observation sounds like a polite reprimand to the mathematics of his day and even more so to the mathematics of today.

3.5 The world as a strategic game: a mathematical idea of rationality

Von Neumann's confidence in the power of mathematics and his acceptance of the philosophic principles underlying classical science were partly due to his own nature and character. This aspect has been pointed out by Heims, who interviewed several living contemporaries of von Neumann. Other accounts can be found in the 1966 Mathematical Association of America film *John von Neumann – A documentary*. The interviews do not give us a univocal picture of the man, although they do shed some light on several aspects of his personality, his working style and his world view. Von Neumann had a fundamentally optimistic and positive personality, abounded in energy and initiatives and was a great worker; at the same time, he was well-organized and circumspect. For him, mathematical work was a true pleasure, and certainly the pleasure of a *virtuoso*, if we take into account his extraordinary ability and command

of the subject. In this connection endless anecdotes are told. Indeed he may actually be considered a living "legend" in the scientific world. For instance, the analyst Peter D. Lax, professor at the Courant Institute of Mathematical Sciences of New York, tells of when, during a university lecture, he was unable to complete the proof of a theorem, von Neumann commented: «I knew three different ways of proving this result, but unfortunately I chose a fourth way». And he went on: «The popular saying was: "Most mathematicians prove what they can, von Neumann proves what he wants".»[66] Emilio Segrè, one of von Neumann's colleagues of Italian origin at the Los Alamos Laboratory during World War II, told the following anecdote:

> *It happened at Los Alamos. Prof. Hans Staub and I needed an integral for a calculation* (*probably a solid angle in some instrument*) *and had written it on a blackboard. We had racked our brains for a good while without getting anywhere. Von Neumann, who was a friend of ours, happened to pass along the corridor, saw the integral through the open door, and gave us the numerical value off the cuff, continuing on his way after saying hello.* [...] *At Los Alamos, von Neumann and Fermi every now and then would put up two blackboards and compute hydrodynamics problems and I believe they would have a race to see who was faster. They would both get to the end without even a numerical error, but my impression is that von Neumann was the faster.*[67]

A methodical worker and possessing great powers of concentration, von Neumann became famous for the extraordinary speed with which he could solve a wide range of problems. In this an important role was played by his ability to formulate the problems theoretically in the most appropriate way so as to identify their most salient features. A man with great intellectual capacity and powers of abstraction, he was also a practical man of the world.[68] Instead of the typical image of the eccentric and unsociable mathematician or physicist, the image he projected was that of a balanced person whose constructive attitude enabled him to overcome both the vicissitudes of his personal life and the challenges of scientific research. He combined enthusiasm and intellectual curiosity with a dispassionate, objective, highly rational attitude, and earned himself the reputation of an ideal scientist in the international community. As Heims put it:

66) Glimm, Impagliazzo, Singer, (eds.) 1990, 5–6.

67) Letter sent to G. Israel, 1 February 1987.

68) There are also numerous anecdotes about von Neumann's social life, about the parties he used to give and above all his passion for fast driving, his mania for telling dirty jokes and other vulgar attitudes. There is an enduring impression that these anecdotes were strongly influenced by a dislike for the man because of his conservative and "war-mongering" political views. It is no coincidence that greater credence in this sense was given in opposite political circles and that a similar situation occurred in the case of Edward Teller, who was indeed sometime presented as a true monster. We consider that these stories are marred by prejudice and thus relatively unreliable, and we are therefore reluctant to give them much credit.

> *This logical mastery also may have affected von Neumann's views and premises concerning the world. It became his mathematical and scientific style to push the use of formal logic and mathematics to the very limit, even into domains others felt beyond their reach. He seemed to regard the empirical world, probably even life and mind, as comprehensible only in terms of abstract formal structure.* (Heims 1980, 129)

His reductionist panmathematical conception was to guide him in the scientific projects we will be describing in the following chapters of this book. However, at the outset, several considerations should be noted concerning the spirit underlying them, in order to situate them within this scientific conception. One of the first non-physical applications that he was to consider was the social sciences; this was in harmony with his social and family background, and with his familiarity with the Vienna Circle project aimed at the unification of science (including all its branches, from economics to ethics and psychology). He conceived of the interaction among individuals as a process that could be schematized in abstract terms and reduced to a comparison among conflicting rational strategies that needed to be brought back to a state of compatibility. Following this approach, all moral or hedonistic considerations disappeared, the world view of the rational individual left no room for the consideration of time (above all historical time), and economic and social action was considered to be a chapter of game theory. His interest in game theory and, more generally, his attempt to represent rational subjective behaviours in mathematical terms, displays an obvious relation of continuity with the more ambitious project that concerned him towards the end of his life: the study of the human mind, the impenetrable mystery underlying the exposition of any theory of nature, society and man. If this project was to be addressed, according to von Neumann, the key was once again to be mathematics. Let us for a moment dwell on a passage from the 1954 Princeton lecture which is particularly enlightening in this regard:

> *Another thing about which we can't tell today as much as we would like, but about which we know a good deal, is that it might have been quite reasonable to expect a vicious cycle when one tries to analyse the substratum which produces science, the function of the human intelligence. The whole evidence of exploration in this area is that the system which occurs in intellectual performance, in other words in the human nervous system, can be investigated with physical and mathematical methods. Yet there is probably some kind of contradiction involved in imagining that at any one moment, an individual should be completely informed about the state of his nervous apparatus at that particular moment. The chances are that the absolute limitations which exist here can also be expressed in mathematical terms, and only in mathematical terms.* (Neumann (von) 1954a, in JNCW, vol. 1, 486–487)

This passage is very important and profound. It reveals his awareness of the great difficulties inherent in the idea of studying subjective behaviour in objective terms, and

indeed that the principal difficulty involved is practically insoluble, as it boils down to the circular idea that a subject can be completely informed about the state of its neuronal apparatus by using the apparatus itself. Not only that: he actually claims that his is an *absolute* limitation. However, this leads to the redeeming feature of mathematics: only mathematics can preserve the hope of dominating this vicious circle by allowing these limitations to be expressed in axiomatic terms or, more precisely, by incorporating these limitations in the model's formal structure.

At this stage, two different considerations may be noted. The first is an observation: von Neumann is far from thinking that there may be an objective and complete description of mental and subjective behaviour: The second consideration is more of a question: does this view represent just one stage of his life or was it already present from the outset? We shall see later, in particular in Chapter 5, how further studies of the brain, computers and automata, as well as of game theory, led him to adopt an increasingly cautious and even sceptical position regarding the possibility of constructing a descriptive theory of the mind based on the physical-mathematical model. Moreover, the tone of the passage cited suggests the influence of lessons drawn from Gödel's theorem. In spite of this, a clear continuity link is present also here. It is sufficient to go back to what we have seen in the case of game theory: on the crucial issue of mixed strategies, he takes an almost exclusively rationalist-normative approach, in complete contrast to Borel who follows a psychologistic-descriptive approach. For von Neumann it is not a matter of describing the behaviour of the players *as if it were* reality, but rather of determining the conditions in which there exists a solution to a strategic conflict that takes the opponents' needs into consideration as far as possible.

After all, what distinguishes Borel from von Neumann is a radically different way of conceiving of *rationality*. Borel seeks the best rational behaviours in the players' concrete procedures, in manifestations of the perfection of their "ability" displayed in a strategic confrontation. Von Neumann seems to be uninterested in and sceptical about true spontaneous behaviours. He is looking for *definitions* of an ideal behaviour that will afford a solution to the conflict in the least disadvantageous way for all, that is, which achieves the greatest number of aspirations of each person involved that is compatible with the limitations imposed by the presence of an opponent. It would be vain to object to von Neumann – from Borel's point of view – that these are not real behaviours: his response to this kind of objection would be (and indeed was) that a scientific solution to the problem could be obtained only in quantitative and mathematical terms, and that the latter pass through a representation in axiomatic terms of an "optimal" behaviour of the subject that can lead to a solution. Thus, the point was to represent the limitations of the problem «in mathematical terms, and only in mathematical terms»... It would be a great misunderstanding to think that von Neumann pursued the project of drawing up a map of human rationality: despite all the changes and the substantial in-depth investigations emerging from his subsequent scientific research, the regulatory and panmathematical view is already present in his first work on game theory.

It is symptomatic to observe that this conflict between normative view and descriptive view – the dialogue between these two points of view has always been a

dialogue between the deaf [69] – was to recur in the interaction between von Neumann and Norbert Wiener. Two different views of what is rational came again into play. Like Borel, Wiener aimed at a description of real behaviours, and believed he had found the key in an idea that is remote from Borel's player's psychology: this is the concept of *homeostasis*, which for Wiener was the substance of the behaviours of the subject, both considering the life of the mind or the actor in the social scene (Israel 2004c). While Borel was far from holding a reductionist approach, on the opposite side Wiener was even more reductionist than von Neumann, because he believed in the identity of man and machine, that is, he identified brain and automaton, and considered that the essence of this identity actually lay in the concept of homeostasis. He therefore found von Neumann's game theory doubly abhorrent: because it did not rely on the concept of homeostasis, and because it was based on an idea of rationality devoid of any real foundation. In the repeated controversial comments he made on von Neumann's game theory starting from his influential 1948 essay *Cybernetics, or control and communication in the animal and the machine* (2^{nd} edition 1961),[70] Wiener emphasized this lack of realism:

> *This theory is based on the assumption that each player, at every stage, in view of the information then available to him, plays in accordance with a completely intelligent policy, which will in the end assure him of the greatest possible expectation of reward. It is thus the market game as played between perfectly intelligent, perfectly ruthless operators.* [...]
>
> *Naturally, von Neumann's picture of the player as a completely intelligent, completely ruthless person is an abstraction and a perversion of the facts.* (Wiener 1961, 159)

This abstract "perversion of the facts" is exemplified by Wiener in the conflict between Napoleon and the Austrians in Italy, in which the former adopts a particularly bold and completely rash strategy based on the conviction that the Austrian strategists were totally unaware of the new methods of waging war of which the French army was capable. Likewise, admiral Nelson would not have defeated so resoundingly the French fleet if he had started from the axiom that he was facing an adversary of equal capacity and experience. Wiener comes to the severe conclusion that «any direct use of the von Neumann method of game theory in these cases would have proved futile» (Ibidem, 171).

69) One example of the incomprehension between these two points of view which displays very interesting analogies is that of the discussion between Henri Poincaré and Léon Walras regarding the possibility of mathematizing economics: for Poincaré the assumption that economic subjects are infinitely farsighted is a comparatively non- credible description while for Walras it is a matter of intervening in a regulatory fashion in order, at least tendentially, to make economic subjects increasingly more farsighted. See Ingrao, Israel 1990, 154–161.

70) Several passages from Wiener 1961 cited here are found almost verbatim in *God & Golem, Inc.* (Wiener 1964) and several controversial ideas are already also present in *The human use of human beings* (Wiener 1950).

As Wiener writes: «The von Neumann type of approximate theory tends to lead a player to act with the utmost caution, assuming that his opponent is the perfectly wise sort of a master» (Ibidem). In actual fact, von Neumann's idea of rationality is based on an element of caution. It's quite natural to wonder what were the roots of this view of rationality as caution. In our view, they are of both of an exogenous nature and dictated by the logic of his scientific views.

There is no doubt that von Neumann, even with the inevitable fluctuations, did not hold an optimistic view of the rationality of human beings, defined as the capacity to adopt solutions capable of reducing damage to a minimum. At most – as we shall see in Chapter 4 – he had occasion to say that mankind has a certain capacity for overcoming the problems it has itself caused after "varying amounts" of suffering. This sceptical attitude stemmed from serious and deep-seated reasons in his own life experience, during which, from an early age, he had witnessed the series of catastrophes that had destroyed his country, and then the global catastrophe of Europe, the World War, and the Cold War. It was difficult for him, and for all those who had lived through similar experiences, to believe in the innate rationality of human beings. Therefore there was very little that was rational to be described in reality... If anything, there was a rationality that must be imposed. And this "reasonable" rationality could only entail a search for the maximum possible advantage that was however compatible with the analogous goals of the others. The catastrophe could thus originate from an excessive claim, from imprudence, from the lack of self-limitation. Rational meant merely pursuing the minimum possible harm.

It should be noted that such a moderately pessimistic attitude, tempered exclusively by the quest for a solution resting in scientific rationality, appears from the outset in attempts at the scientific analysis of human behaviour. In the eighteenth century, the founder of the Physiocratic sect, François Quesnay, wrote: «Let us not seek lessons in the history of nations or human dismay, which portrays to us only an abyss of disorder».[71] The lesson must rather be sought in scientific normativity. In this sense, von Neumann is clearly a descendant of enlightenment rationalism and of its project for a scientific management of society.

But – and this brings us to the internal aspect regarding the scientific method – how is this normativity implemented whenever it attempts to establish itself on the instruments offered by mathematics? As we shall see, von Neumann was an explicit champion of the idea that the mathematization of social and mental phenomena needed a "new" mathematics, just as classical mathematical physics – during the seventeenth and eighteenth centuries – needed to create a new mathematics *ad hoc*, that of differential and integral calculus. However, even if the methods created by von Neumann were deeply innovative, if compared with those of classical mathematical analysis, mathematical reasoning offers a comparatively univocal way of formulating the normative approach, namely, the idea of *optimization*: the imposition on reality of a quantifiable objective, in this case the attainment of the greatest possible advantage, even in the presence of constraints. Furthermore, at the earlier stages, the analysis of

71) Quesnay 1767. In this connection, see Ingrao, Israel 1990, 43; see also Israel 1993a.

social problems cannot avoid considering the behaviours of the individual subjects as homogeneous, because the total differentiation of these behaviours would make the problem so complex as to be unmanageable. In essence, the idea of physics reappears: to disregard the specific characteristics of individual objects, which Galileo referred to as "setting aside the impediments". Von Neumann's critics might be tempted to say that in this way subjective behaviours are described in a schematic and caricatural way vis-à-vis reality. However, the response to this remark would be similar to the one given by von Neumann to Borel (as we have seen in Chapter 2) when he said that a game theory without the minimax theory was inconceivable: is it possible to have a mathematical treatment that is not based on comparatively radical abstractions? The subsequent discussions of this issue continue to the present day – and in particular Nash's game theory, of which more will be said in Chapter 5 – not only suggest a negative answer to this question, but even point to a much stronger axiomatic rigidity than that proposed by von Neumann.

The foregoing explains the meaning of the title given to this book. For von Neumann, the world must be *conceived of as* a mathematical game, in the sense that in all cases it is useful and effective to seek axiomatic structures suitable for thinking of the phenomena in mathematical terms. The concept of strategic game is a kind of universal key for considering in terms of combinatorial structures all the interactions occurring in reality and to determine the conditions in which they allow an "acceptable" solution. But this in no way means that the world *is* actually a mathematical game. The conception of social interactions as a game, the combinatorial view on which the theory of automata will be founded, the analogy between brain and computer are tools of epistemological analysis, never ontological views.[72]

This conception of von Neumann was deepened and enhanced in the United States, also thanks to a growing involvement, together with economists, engineers and other researchers in the new postwar technological challenges. Those years, also with his fundamental contribution, saw the development of a science-technology-industry-military conglomerate that marked the historical-cultural evolution in the

72) It is also this difference between the ontological and the epistemological approach that distinguishes von Neumann from Wiener, despite their mutual esteem and numerous collaborations. In the 1940s and 1950s von Neumann showed interest in Wiener's activities, which culminated in the cybernetics program. We have illustrated the diffidence shown by the former regarding the teleological approach, which he considered was not independent of the causal approach. As we shall see in Chapter 5, in early 1945 Wiener promoted a working group that he wanted to call the Teleological Society. Von Neumann was a member but was accused in a letter Wiener wrote to Arthur Rosenblueth of being a "very slick participant" (quoted in Aspray 1990, 316). It is difficult to shrug off the impression that von Neumann's coolness was linked to a conceptual disagreement. We have seen how the operation von Neumann developed in physics limited the crisis affecting the discipline, but in fact his intervention contributed to opening a gap between physics and mathematics. Wiener was the prophet – not fully understood in his time – of a new physics that was to extend its boundaries to take in phenomena in which uncertainty appears, to phenomena of complexity and of incomplete information: to achieve this aim new mathematical tools were necessary that would allow order to be identified in disorder. Averages constructed on the basis of Lebesgue's integral and other probabilistic concepts would allow this to be done and, for this purpose, it was necessary to include time as a fundamental aspect in the description of phenomena.

second half of the twentieth century. Those years also witnessed construction of the "systems approach", which was strongly marked by mathematical ideas and techniques and in particular by game theory. Systems thinking was based on key words that von Neumann himself already identified with clarity in his review of Wiener's book *Cybernetics*, published in *Physics Today* in May 1949:

> [. . .] *the proposition that science as well as technology, will in the near future and in the farther future increasingly turn from problems of intensity, substance, and energy, to problems of structure, organization, information and control.* (Neumann (von) 1949a, 33)

These were the words of a man of science who, in the meantime, had been treading the complex corridors of power in his new country.

Chapter 4

Von Neumann in the United States

4.1 Princeton and the American mathematical community in the 1930s

In September 1928, von Neumann attended the International Congress of Mathematicians held in Bologna as a member of the German delegation. One of the most celebrated lecturers at the Bologna Congress was Oswald Veblen, a distinguished member of the United States mathematical community.[73] The meeting with Veblen radically changed the course of von Neumann's life, taking him definitively away from his native land towards new professional and personal horizons. Veblen, who was professor of mathematics at Princeton University, had distinguished himself for his contributions to the axiomatization of projective geometry. In addition to his scientific activity, throughout his life he engaged in an intense organizing and institutional activity in his country, which deeply influenced not only the development of American mathematics but also the professional career of many mathematicians. His efforts were aimed at making mathematical research in the United States independent and competitive at the international level by adequately boosting university teaching of the subject. This goal had been pursued in many other countries – including Hungary, as illustrated in Chapter 1 – in the wake of successful educational reforms introduced in nineteenth century Germany. The German reforms had led to establishment of a high quality network of secondary schools and an organization of engineering education that was the envy of the whole world. They had also attributed a leading role to the

73) *Atti del Congresso Internazionale dei Matematici, Bologna,* 3–10 *settembre* 1928, Bologna, Zanichelli, 1929–32, 6 vols.: vol. 1. On Veblen and the development of the American mathematical community, see Parshall, Rowe 1994 and the references therein, also with regard to the influence of the German model on this development. See also Duren (ed.) 1988, and, on Princeton, Aspray 1988.

university, redefined in a modern sense as the place where new frontiers of knowledge were expanded thanks to free research and the training of young researchers.

Until the end of the nineteenth century, most American universities had a standard of teaching barely higher than that of secondary schools in Europe. The majority of the lecturers had a very heavy teaching load and gave courses at elementary levels. There was no form of funding of scientific research and indeed research and publications were not considered either as an obligation for or as a merit of the professors. The first university to break with this tradition was the Johns Hopkins University of Baltimore which, right from its foundation in 1875, had devoted serious attention to postgraduate and research training. In the field of mathematics, this university relied on the British mathematician James Joseph Sylvester who, during his stay in the United States between 1876 and 1883, gave a strong impetus to research. Acting as an advisor for young graduates, he set up an intellectual research school in mathematics; furthermore, he founded the first American review in the field, the "American Journal of Mathematics", which began publication in 1878. The example of Johns Hopkins was soon imitated by several of the older universities, such as Harvard, Princeton, and Yale, and by others founded towards the end of the century, such as Clark University or the University of Chicago. Many of these universities were later to vie with each other to attract high-level mathematics professors.

In 1888 three young graduates of the New York University founded the Mathematical Society of New York, based on the model of the London Mathematical Society. The aim of the Society was to bring into contact persons interested in mathematics belonging to a wide range of environments, such as university professors or high school teachers, actuaries, and engineers. The rapid increase in membership attested to a growing interest in mathematics in the whole country: in 1894 the society changed its name to the American Mathematical Society (AMS).

However, decades were to pass before the home-grown research schools were consolidated into a mathematical community. Sylvester's influence waned rapidly after his return to Britain. The majority of future American university professors of mathematics completed their training in Europe, especially in Germany. The main pole of attraction at the time was Göttingen, and the favourite advisor for preparing a dissertation was Felix Klein, who was always open to collaboration. The importance of Göttingen and of the protective figure of Klein in the process of forming American mathematical research culminated symbolically in his participation at the Mathematics Congress of Chicago in 1893, on the occasion of the Colombian Universal Exposition. The Congress organizers were the three professors of mathematics of the University of Chicago: two of them were Germans and pupils of Klein and the third was Eliakim Hastings Moore, who later on took over Klein's role as leader and animator of mathematics in the United States. As Parshall and Rowe have pointed out, «between 1896 and 1907, in fact, the list of Moore's students reads like a *Who's who* in early twentieth century mathematics» (Parshall, Rowe 1988, 26).

One of Moore's most brilliant students at Chicago was actually Veblen who was taken on at Princeton University in New Jersey in 1905. Princeton had succeeded in attracting a good number of excellent mathematics professors, but nevertheless in the

early 1920s it was still mainly a teaching centre where no provision was made for research activity. Veblen devoted all his energy quite successfully to changing this state of affairs, both at local and national levels. At the national level, in the two years 1923–24, as president of the AMS, he made considerable efforts to obtain government funding for the Society's publications and activities; he also managed to have mathematics included in the study grant program of the National Research Council (NRC), the institution set up at the end of World War I that – as in similar institutions in other countries – set out to place on a new basis the relations between scientific research, technology, and industrial development. At Princeton, one of Veblen's objectives was to organize a research institute in which a group of top flight mathematicians could work. It was an ambitious idea: he not only wanted to establish research positions but also to reproduce the organizational model of Göttingen. The favourable economic cycle seemed to offer excellent prospects: the Rockefeller Foundation General Education Board offered a million dollars for the development of basic research at Princeton University, on condition that the University find a further two million, which were actually collected in 1928, thanks to numerous donations. The Rockefeller Foundation is one of the best examples of a truly US tradition of offering private donations to encourage academic studies and research – a kind of support scarcely available in continental Europe. One important benefactor of Princeton University was an alumnus, Thomas Jones, a lawyer and president of the Mineral Point Zinc Company. In the years leading up to the Great Depression, Jones donated funds for research, for construction of a building to house the department of mathematics and for establishment of chairs, including one for mathematical physics. The Jones Chair was occupied in 1928–1929 by Hermann Weyl, who returned to Germany immediately afterwards to succeed Hilbert in the chair at Göttingen. After Weyl's departure, at the proposal of Veblen, it was decided to use the funds available to invite foreign professors. Veblen's candidate was von Neumann, and the faculty decided to invite both him and his fellow countryman and childhood friend, Wigner.

Veblen personally supervised the proposal, design and construction of the new premises of the department of mathematics, completed in 1931 and called Fine Hall, in memory of Henry B. Fine (who was the director of the department until 1928). Fine had obtained his PhD under the guidance of Klein and was a profound admirer of the German university system: Veblen honored his memory by organizing a research group that faithfully pursued the project developed by Klein at Göttingen. The library, lecture rooms, studies and communal rooms offered the best possible conditions for study, research and informal contacts among professors and students. The professors recruited, who included many foreigners, the fellowships for young graduates, the funds for the guest professors, all contributed to recreating at Princeton the stimulating cosmopolitan environment von Neumann had known in Göttingen. Moreover, we have recalled in Chapter 1 von Neumann's deep attachment to Hungarian mathematics and to German mathematics; but for a young mathematician such as von Neumann, trained in Hilbert's entourage, the whole cultural world was indeed a single country.[74]

Von Neumann's early stays as guest professor at Princeton took place in the gloomy period of the economic and financial crisis that exploded on the famous Black Thursday on the New York Stock Exchange in October 1929. And yet, the situation in Europe, and in Germany in particular, was even more dramatic. After several years on the margins, Adolf Hitler's National Socialist party enjoyed a growing consensus and in September 1930 obtained a considerable increase in votes for the first time. In the years that followed – in January 1933 Hitler was given the responsibility of forming a government, and in March he won the election – the idea of remaining at Princeton must have appeared more promising to von Neumann than any other possibility. In April 1933, in a letter to Veblen, he asked about the atmosphere in the US and described the process of what he described as the "purification" of German universities:

> *I am glad to learn from your letter that these things receive the full attention and appreciation in America which they deserve. It is really a shame, that something like that could happen in the 20th century.* [...] *We are going to Italy for 3–4 weeks around May 10, and then for 2 weeks, or less, to Germany. I feel that I have to see Berlin and Göttingen once more – although an expedition to the North Pole would be a much nicer thing under the present conditions.*[75]

Veblen, for his part, was relying on von Neumann for his most ambitious projects: to create at Princeton an institute dedicated exclusively to research and teaching in preparation for PhDs and postdoctoral studies. The great occasion had come in 1930 thanks to Louis Bamberger – owner of a large department store and leading citizen in a town near Princeton, Newark – and his sister, Caroline Bamberger Fuld. They offered to fund an advanced research institute, according to a proposal made by Abraham Flexner, an expert in educational matters who had worked for the General Education Board of the Rockefeller Foundation. Flexner, who was appointed director, chose Princeton and mathematics as the first area of activity of the new Institute of Advanced Study (IAS). After two years of preparation, Veblen was elected as the first professor of the Institute. His ideas influenced Flexner who decided, together with him, on the names of the other professors to be appointed. The headquarters of the IAS was at Fine Hall, until a special building, Fuld Hall, was opened in 1939.

While the cultural tradition of Göttingen, Rome and many other centres of continental European science was being ravaged, first by racist purges and then by the war, in Princeton and in many other places in the United States, an extraordinary environment was being created for mathematical research. The United States was taking on the role of world leader of research and higher scientific and mathematical learning. An outstanding contribution to this process was made by the flow to the country

74) Hilbert's famous words were: "For mathematics, the whole cultural world is a single country" (quoted in Reid 1973, 188). On Hilbert's internationalist defense of the participation of German mathematicians to the 1928 Bologna Congress, see Segal 2003, 365–366.

75) Neumann (von) to Veblen, April 3, 1933, O. Veblen Papers, Library of Congress, now in Rédei (ed.) 2005, 264.

of a great number of European mathematicians, who slowly reconstructed a fabric of scientific and academic relations and were integrated into local institutions. The IAS in particular became an important crossroads, owing to the presence not only of researchers employed by the Institute or the University but also of many others that had taken refuge in the country and paid frequent visits to Princeton.

4.2 A lucky migrant

There was an immediate reaction in the United Kingdom and the United States to the sacking of university professors that began in Germany in April 1933. Also Turkey and several Latin American countries, which in those years had been committed to higher education reforms, opened up to migrants. The situation worsened after the invasion of Austria and Czechoslovakia in 1938, the promulgation of the racial laws in Italy the same year, and the outbreak of war.[76]

In the United States, the Immigration Law accorded professors exemption from the fixed immigration quota system, facilitating the entry of those who had been invited by an institution in that country. However, the economic crisis of the Great Depression years considerably complicated the integration of European scientists into the country's structures. The Emergency Committee for Displaced German Scholars, later expanded into that for Displaced Foreign Scholars, in many instances with the support of the Rockefeller Foundation, offered numerous temporary posts to those who did not have a direct contract with a university. The Committee was obliged to follow a cautious policy owing to the economic recession. In October 1933, according to the Committee's estimates, out of a total of 27,000 American university professors distributed among some 240 institutions, 2000 had lost their jobs. The policy followed in the granting of aid was thus dictated by several different factors: to encourage scientific excellence, to avoid refugees taking the place of local teaching staff, and to avoid the rise of nationalistic xenophobic reactions and anti-Semitic feelings. Consequently, the majority of the grants were of two years duration and without the attribution of ordinary teaching tasks; in other words, the foreign professors were to concentrate on research, and to give only postgraduate courses. Nevertheless, despite the good intentions, the opposite effect was obtained, as mistrust of the new arrivals – who were apparently threatening the jobs of young American researchers – was added to the impression that they enjoyed a privileged status whereby they were exempt from the heavy teaching load of American universities.

Veblen was one of the key figures in the organization of the emigration of mathematicians in collaboration with the Emergency Committee and with the advice of Hermann Weyl. The AMS undertook, at Veblen's behest, to integrate the foreigners into American mathematical institutions as painlessly as possible. And despite the difficulties due to the pressure of many young mathematicians who were jobless or were employed exclusively in teaching posts, the determined rejection of Nazi pol-

76) For the United States, see Reingold 1981; on the migration of German scientists to Turkey under the presidency of Kemal Atatürk, see Widmann 1973.

icy by the majority of American mathematicians contributed to the refugees being accepted. The most important response to the Nazi academic policy was a decision by the AMS in 1939 to set up a journal, *Mathematical Reviews*, as an alternative to the German reviews journal, *Zentralblatt für Mathematik und ihre Grenzgebiete*, published by Springer, the policy of which had yielded shamefully to Nazi impositions by excluding Jewish or politically suspect scientists.[77]

In this difficult situation, von Neumann's early appointment as an IAS tenure faculty member in 1933 ensured him a secure and comfortable position. His career as a mathematician developed smoothly during the 1930s. The first course of lectures he held at the Princeton University as Jones professor for mathematical physics in the Spring of 1930 was devoted to quantum statistics. The following year he dealt again with questions of mathematical physics, and in particular hydrodynamics. After his appointment to the IAS he continued to hold courses that followed the paths of his own research, and the relative cyclostyled notes were widely circulated among American mathematicians: measure theory (1933–34); the theory of operators (1934–35); continuous geometry (1936–37); invariant measures (1940–41). These were innovative postgraduate courses closely linked to ongoing research and yet he did not have the time to see to their publication as textbooks. They were published several years later, some posthumously: they consisted of the texts *Functional operators* (1950, in two volumes, *Measures and integrals* and *The geometry of orthogonal spaces*), *Continuous geometry* (1960, edited by his former student Israel Halperin) and *Invariant measures* (1999, based on the notes of Halmos, von Neumann's assistant in 1940–41).[78] He worked very hard during this period, encouraged by the favourable atmosphere at the IAS and by his lively intellectual exchanges with his European colleagues. Moreover, little by little, many among these colleagues moved to the United States. For several years he organized together with Veblen a joint seminar on quantum mechanics and geometry. Also, many of the numerous works he published in this period were written in collaboration with other scholars. Quite a few of them were dedicated to questions of functional analysis linked to the physical mathematical research begun at Göttingen.

His first papers in English (the principal language of his publications from 1935 on), published in 1932 in the *National Academy of Sciences Proceedings*, regarded the ergodic hypothesis of statistical mechanics (Neumann (von) 1932b, c). The application of probabilistic considerations to mechanics, particularly to the kinetic theory of gases and thermodynamics, raised numerous controversies during the late nineteenth century, leaving a legacy of numerous open questions: one of the most significant was Ludwig Boltzmann's so-called "ergodic hypothesis" which highly influenced his interpretation of the second law of thermodynamics. The quantum approach had further complicated many facets of research on this topic: in his efforts to incorporate various aspects in a unified interpretation, von Neumann concerned himself with questions

77) Reingold 1988; Israel, Nastasi 1998; Israel 2004d.

78) von Neumann 1950, 1960, 1999. His book *Mathematical foundations of quantum mechanics* (Neumann (von) 1932a) was translated into English only in 1955.

of statistics and thermodynamics. In 1929 he had published in the German journal "Zeitschrift für Physik" a study on the ergodic hypothesis and Boltzmann's "H theorem" in the field of quantum mechanics (Neumann (von) 1929a). He later developed the idea of using measure theory to provide a mathematical formalization of the ergodic theory. Later on the American mathematician George D. Birkhoff improved and extended these results; this marked the beginning of a line of research of great importance both for statistical mechanics and for the theory of dynamical systems.

In this way, ergodic theory began to emerge as an autonomous mathematical theory. Von Neumann continued to take an interest in ergodic theory and discussed it with Wiener (Heims 1980, 171), but he wrote no further articles on this topic. He instead continued to study the theory of operators in Hilbert spaces, obtaining a general formulation of spectral theory. In 1929 he had introduced for the first time families of operators characterized by a certain property, based on the research of Emmy Noether and Emil Artin on non-commutative algebras. Many years later he proposed this topic to a newly minted Columbia PhD, Francis J. Murray, who in 1935 won an NRC grant to complete his training at Princeton: von Neumann and Murray thus developed together the study of rings of operators, later known as von Neumann algebras.

This research stimulated an interest in the study of spaces that generalize projective spaces related to lattice theory, which was given the name "continuous geometry". This approach aroused the interest of Garrett Birkhoff and, in 1936, he and von Neumann collaborated on an article that proposed an innovative approach to the foundations of quantum mechanics.[79] In the summer of 1935, Birkhoff, von Neumann, and Marshall H. Stone, on their way to a mathematical congress in Moscow devoted to topology, presented these results in the course of several lectures given at Warsaw.[80]

Another important line of research in the field of measure theory and related to Hilbert's fifth problem – consisting in the conjecture that each locally Euclidean group is a Lie group – was suggested by an article by his Hungarian colleague Haar demonstrating the existence of an appropriate measure in topological spaces. Von Neumann read Haar's paper in 1932, while it was still being written. It was published in 1933, in the Princeton *Annals of Mathematics*, together with a solution by von Neumann for Hilbert's fifth problem for compact groups, which used Haar's measure. This result aroused considerable interest in Moscow and, in 1936, von Neumann published another article on the uniqueness of Haar's measure in the first issue of the new series of the USSR journal *Matematičeskij Sbornik*.[81] These studies were followed by other articles on quasi-periodic functions, written in collaboration with Solomon Bochner.[82]

79) Neumann (von), Birkhoff 1936. See Van Hove 1958 and Rédei, Stöltzner (eds.) 2001.

80) Ulam 1958, 19. For further details see Birkhoff 1958 and the complete bibliography of von Neumann's works in JNCW 1960–1963.

81) Haar 1933, Neumann (von) 1933 and 1936; on von Neumann's work on measure theory see Ulam 1958 and Halmos 1958 and 1973.

This astonishing succession of topics was tackled by von Neumann at a hectic rate, without dedicating too much time to any one of them. Garrett Birkhoff, for example, wondered what degree of development lattice theory would have attained if that intense activity had lasted twenty years instead of two or so (Birkhoff 1958). And Halmos, after pointing out that von Neumann's publications on measure theory and ergodic theory represented one eighth of his scientific production, wrote that: «it seems to be safe to say that if von Neumann had never done anything else, they would have been sufficient to guarantee him mathematical immortality» (Halmos 1958, 93).

His prestige was now consolidated: in 1937 he was invited to hold the oldest and most prestigious cycle of lectures of the AMS, the "Colloquium Lectures", and in 1938 the Society awarded him the Bôcher Prize. But, in the meantime, throughout Europe – from Moscow to Göttingen, from Budapest to Rome – the dramatic developments of the political situation were either drastically curtailing scientific activity or impressing a new orientation on it. The outbreak of World War II and the subsequent involvement of the United States interrupted the peaceful course of academic life at Princeton and marked a turning point in von Neumann's life and scientific career.

4.3 Scientific commitment during World War II

As soon as he obtained US citizenship in 1937, von Neumann received, via Veblen, a proposal to collaborate with the US armed forces. The Ballistics Research Laboratory (BRL) of the Army Ordnance Department (AOD), located at Aberdeen, Maryland, was being reorganized in view of the pre-war situation in Europe. Veblen, who had collaborated with the Laboratory ever since its foundation during World War I, considered that military research could take advantage of the contribution of civilian scientists.

At the time of World War I, scientists' contributions to wartime technology had attracted the attention of governments to the role of science and the scientific mind in this context. It was in this period that the NRC was founded in the United States in order to organize government participation in the development of scientific and technological research. Similar bodies had been set up in many European countries. The interest of governments in the technological role of science continued after the war, side by side with that of private industrial corporations, such as American and German companies developing electricity and the telephone. However, the outbreak of World War II led to a quantum leap in this direction, above all because of the effectiveness of scientific ideas and research in the development of artillery and other aspects of military technology, such as cryptography, radio transmissions and radar or various vehicles such as aircraft, tanks and submarines. Indeed, this conflict marked the establishment of an iron pact between the science and engineering community and national defense, especially in the United States, where scientific and technological development was considered, from that time on, as necessary for the nation's progress

82) Neumann (von) 1934; Neumann (von), Bochner 1935. Bochner himself was a refugee mathematician; he was born in Podgórze (now Poland) and spent his early career in Germany.

and to guarantee its security. The US government agencies – above all the military ones – became the main source, and more generous than ever, of research funds.[83]

At the Aberdeen Laboratory, von Neumann came into direct contact for the first time with the needs of "applied" military technical problems and established his first direct relations with military institutions. There is no doubt that his "refugee's" sensitivity and his explicit liking for a country that had welcomed him with open arms formed the basis of the willingness he immediately displayed to collaborate in military research:

> *I must say that the main reason* [*for his coming to America*] *was partly because conditions in Hungary were rather limited, and I thought the thing I was doing had a better field in America and to a considerable extent because I was much more in sympathy with the institutions of America; and lastly, because I expected World War II, and I was apprehensive that Hungary would be on the Nazi side, and I didn't want to be caught dead on that side.*[84]

To this must be added that, throughout his scientific career, he had displayed great receptiveness to the use of mathematics in a wide range of different contexts. Problems related to improving artillery shells, the study of shock waves and similar problems were linked to questions of mathematical physics which aroused his curiosity above and beyond any consideration of practical utility.

In the Autumn of 1938 he made his last trip to Europe, visiting Lund, Copenhagen and Warsaw. In his letters to Veblen, he spoke of the red tape necessary for organizing his hasty marriage with Klára Dán , the daughter of a Jewish family in Budapest. In October, the future bride obtained her divorce and on 17 November they got married in their native city, leaving immediately afterward for the United States. The whole family, mother and brothers and sisters, followed them soon after. The United States was a safe haven and the work continued. An intense collaboration with the economist Oskar Morgenstern, after the latter's arrival at Princeton in 1938, resulted in an article so long that the authors decided to publish it in book form. The manuscript was completed in January 1943. The book, *Theory of games and economic behavior*, published in 1944, received favourable reviews and praise, including

83) See Moy 2001 and Kevles 1995 as representative of the considerable research on these topics carried out in recent years which has shed light on the origins of the so-called military-industrial-university complex of the Cold War period. On World War I, see the pioneering study by Hughes 1971; and on industrial research, Smith 1990. See also Ronald Kline's analysis (Kline 1995) of the cultural discourse among engineers and scientists that accompanied the development of this complex, whose point of arrival is the well-known *Science, the endless frontier: A report to the President on a program for postwar scientific research* by Vannevar Bush, drawn up within the OSRD (Bush 1945).

84) Quoted by Aspray from a hand-corrected draft of testimony "Nomination of John von Neumann to be a member of the United States Atomic Energy Commission", 8 March 1955, conserved in the Library of Congress (Aspray 1990, 256, note 32); compare Marina von Neumann Whitman's testimony quoted in Chapter 1.

one in March 1946 in the New York Times. It soon became a science best seller in spite of the unusual new topic it treated.[85]

However, from the beginning of the war in Europe and above all after the United States entered the conflict in December 1941, military research absorbed a growing share of von Neumann's time, leading him to neglect his scientific projects. The atmosphere that prevailed at that time is reflected in a few lines he wrote to Ulam:

> *As to the war: I think, as before,* qu'on les aura. *May take honorable* 2-3-4 *years. But it's going to be a peculiar little world* [. . .] *Life is getting complicated. I am indeed writing a book on "economics" with O. Morgenstern* (*at Princeton University, he was formerly the director of the Austrian "Institut für Konjunkturforschung"*). *It is, as you guessed, mainly on games, with a tendency to apply it to oligopoly etc. and with a distant hope of some application to social phenomena.* Qui vivra verra.
>
> *But I'm getting more and more snowed under by war work.*[86]

In 1940 he was elected member of the newly established Scientific Advisory Committee of the Aberdeen Laboratory. That same year, together with Robert H. Kent, a distinguished military member of the Laboratory, he wrote a paper that was initially published as an internal report and then in the *Annals of Mathematical Statistics*: the topic treated consisted of an estimation of probable errors starting from successive differences.[87] Again in 1940 he was appointed ballistics consultant in a war preparation committee set up in September 1939 jointly by the AMS and by the Mathematical Association of America (MAA). The committee immediately got in touch with the NRC Physical Sciences Division. The United States was now setting about establishing an organization specifically addressing the war effort.

The first civilian body set up for the purpose of providing systematic scientific assistance to the armed forces was the National Defense Research Committee (NDRC), established in 1940 and presided over by the engineer Vannevar Bush. When the United States entered the war, the initiatives increased in number and were accelerated. Bush moved to preside over a new body having the task of coordinating war efforts and of advising the government on scientific issues, the Office of Scientific Research and Development (OSRD), which incorporated the NDRC, together with other committees dedicated to different matters, such as medical research. The NDRC organized the work of scientists and engineers on such issues as submarine warfare, radar, electronic countermeasures, explosives and rockets. In September 1941 von Neumann began collaborating with Division 8 of the NDRC in the study of detonation waves, aimed at enhancing the preparation of explosive charges in such a way

85) Aspray 1990, 16. See Chapters 1 and 9 of Aspray's book for a rich collection of data on the complex activity of von Neumann in the United States (see the table regarding von Neumann's appointments, consultancies, awards and honorary degrees, ibid., 246–247).

86) Neumann (von) to Ulam, April 2, 1942 (original in English), in S. Ulam Papers, Archives of the American Philosophical Society, Philadelphia, now in Rédei 2005 (ed.), 257.

87) Neumann (von), Kent 1940. The published version is Neumann (von), Kent, Bellinson, Hart 1941.

as to concentrate and direct the physical effects of the blast. In September 1942 he began working in the Mine Warfare Section of the Navy Bureau of Ordnance (NBO).

This job took him first to Washington and then to England, where he stayed from January to July 1943. In those months, he worked in collaboration with the British laboratories on gas dynamics. But above all, it was here that, encouraged by the mathematician John A. Todd, organizer of the British Admiralty Computing Service, he began to concern himself for the first time with numerical questions and the application of automatic computation techniques. «I have also developed an obscene interest in computation techniques» he wrote to Veblen in May 1943, when describing his English experiences.[88]

Even though he had always displayed an interest in the applications of mathematics in investigating physical or economic problems, in this new environment he came into contact with a new aspect of the treatment of mathematics problems: the actual computation of the solutions, which allowed a concrete technological implementation based on theoretical formulations. In the preceding decades, mathematicians and engineers had developed for this purpose many numerical analysis methods using graphical procedures or the kind of algorithms developed for instance by Carl Runge (the incumbent of the first applied mathematics chair set up at Göttingen). Actual calculations were performed by specialized operators known as "computers" or "calculators", many of whom were women, using simple calculating devices manufactured by companies such as IBM (International Business Machines). Thus, the term "computer", which is used nowadays to refer to automatic computing machines, used to refer to persons. Then, in the 1930s, several projects began to take shape concerning the construction of powerful fast machines capable of replacing human operators: first the analog differential analyzers developed by Bush; then the electromechanical calculators of Howard Aiken at Harvard University or George R. Stibitz in the Bell Telephone laboratories – to which must be added, in Germany, the project of Konrad Zuse; and John Atanasoff's electronic calculator at Iowa State University. Wartime needs, such as the calculation of ballistic tables, aircraft design, logistics and fire control led the US government to turn to these pioneers to test the capabilities of the new machines. In the concluding years of the war, this marked the beginning of a new and revolutionary phase of technological development, which led to the modern computer – a stored-program, electronic calculating machine. In this evolution von Neumann was to play an important role.

After his return from England and until the end of the war, he stepped up his activities to an astonishing level. He continued his aerodynamic studies for the Aberdeen Laboratory (where the Hungarian born scientist von Kármán was also working), as well as his collaboration with the NBO and research in Division 2 of the NDRC on the effects of explosive blast and projectile impact on various structures. In September 1943, he began collaborating as a hydrodynamics expert with the scientific laboratory of Los Alamos (New Mexico), where, shrouded in the greatest secrecy,

88) Neumann (von) to Veblen, May 21 1943, O. Veblen Papers, Library of Congress, quoted in Aspray 1990, 27 ff.; see Todd 1974.

the Army Corps of Engineers "Manhattan Project", commanded by General Leslie Groves, had been launched several months earlier. The laboratory was directed by J. Robert Oppenheimer. The aim of the "Manhattan Project" was to construct an atomic bomb that would bring the war to a strategic turning point. Because of his many other commitments, von Neumann was one of the few scientists working on the project who did not reside permanently at Los Alamos, but only travelled there periodically.

One of his first and most decisive contributions derived from the application of his studies on explosives: he persuaded the project managers of the advantage of using an implosion technique to detonate the bomb. Simulation of the implosion required the numerical solution ("integration") of a partial differential equation with two variables. In the beginning, operators worked using desktop calculating machines, but at the end of 1943 an IBM machine using punched cards began to be used. Von Neumann insisted on personally learning how to use the machine. The calculations were very slow and had to be verified manually. Thanks to his huge network of contacts throughout the country, he was able to keep up with the innovations continually emerging in the field of automated calculation and to give advice on how to improve the equipment available at Los Alamos.

Starting in early 1944 von Neumann was also the head of an IAS research contract with the Applied Mathematics Panel (AMP). The main aim of the latter unit, set up in 1942 as part of the NDRC and directed by Warren Weaver, was the solution of complex mathematical problems of a military nature and general coordination of mathematical contributions in this field. The AMP entered into contracts with several universities: the task assigned to the IAS was the study of numerical methods for solving collision problems. In the closing months of the war this contract was extended to research (on behalf of the NBO) on methods to be used in so-called "high-speed calculators". During that period, von Neumann was kept informed of all projects under development regarding automatic machines, in particular that of Stibitz and Aiken's "Mark I" calculator, built with the help of IBM and then donated to Harvard University: this gigantic machine, which so impressed the American public, began to function at full capacity for the Navy after it was completed in January 1944.

However, the most important and top secret project was that of the construction of an electronic machine at the Moore School of Electrical Engineering at the University of Pennsylvania, which was to mark the definitive transition from calculators to modern electronic computers. This was the ENIAC (Electronic Numerator, Integrator, Analyser and Computer, code name "PX"), a project begun in July 1943 by the Aberdeen Laboratory at the request of the AOD. Von Neumann was informed about this project in the late Summer of 1944 by Captain Herman H. Goldstine, a mathematician who liaised between the Moore School group and the Aberdeen Laboratory. When von Neumann visited the Moore School for the first time, the ENIAC project was already at an advanced stage. But before completing the machine, work began on a project for a new machine, the EDVAC (Electronic Discrete Variable Computer), which was supposed to correct the defects discovered in ENIAC. He began systematically to attend the discussions on this new project, which marked the beginning of his great contributions to the development of information science and technology.

4.4 From the Manhattan Project to the Atomic Energy Commission

Many of the scientific projects developed by the various military services did not become operational during the war. When it ended, US scientific and technological research continued to be developed in close collaboration with the armed forces. For instance, the Office of Naval Research (ONR), set up immediately after the end of the war, became the main body supporting mathematical research in the universities. With reference to von Neumann's role, Mina Rees, who had held important posts in the AMP of the OSRD, and who was appointed director of mathematical research at the ONR, wrote:

> *Many of the country's ablest mathematicians were employed on these university contracts, and many moved from their homes to participate.* [...] *John von Neumann* [...] *was also one of those involved with the Panel. But his role, not only during the war but after its conclusion, was unique; for he was a consultant or other participant in so many government or learned activities that his influence was very broadly felt.* (Rees 1988, 277)

The peculiar nature of his role emerges clearly on both the cultural plane, as a key figure in the new configuration of technological systems of the second half of the twentieth century, and on the political-strategic plane, that is, as far as the basis of US armaments policy and of a unilateral approach to foreign policy by many US administrations during the same period is concerned. Despite the many important links between both planes that merged harmoniously in von Neumann's intellectual personality, we shall first consider the aspects related to the military use of atomic energy and the relationship between scientific research and national security. We shall then go on and examine his contribution to creation of the systems approach, which emerged in the context of large-scale technical projects of national security, but which profoundly transformed science and engineering and had a resounding effect on the cultural discourse of the second half of the century.

Von Neumann's participation in the Los Alamos atomic project was not limited to its scientific aspects. From the outset, he kept in touch with military authorities controlling the project and sat on the committee that decided on the tactical aspects of military use of the bomb and location of the objectives to be bombed. The Truman administration's decision to actually use the atom bomb on Japan in August 1945 underscored a change of direction in US foreign policy. In the final months of the war, the country had avoided setting up an Anglo-American anti-Soviet alliance aimed at excluding Stalin's Soviet Union from the management of international political equilibria. The agreement reached in the two international conferences involving the Soviet Union, the United States, and the UK held in 1945, before the war actually ended (the first in February in the small city of Yalta – attended by President Franklin D. Roosevelt, who was to die in April – and the second in Potsdam, in July, after

the Nazi surrender) had for a few crucial months in twentieth century history given a considerable advantage to Stalin, who was able to implement a determined policy aimed at extending the confines of the Soviet Union and at setting up a series of Soviet backed communist regimes in the Balkans and in eastern Europe, and at imposing his control outside his own frontiers also in Turkey, Iran and China.[89]

In eastern Europe, beneath the facade of the coalition governments prescribed by the Yalta declaration and of the "democratic reforms", he carried out a ruthless policy of eliminating all the non-communist political forces existing at the time in countries like Czechoslovakia, Poland, Romania and von Neumann's birthplace, Hungary. Stalin was long able to avoid any radical confrontation with the United States. But starting in early 1946, his expansionist aims produced the consolidation in the Truman administration of a tough stance against the Soviet Union. This was instrumental in bringing to an end the period of Stalin's Realpolitik and marked the beginning of the Cold War, that is, a state of potential conflict that was to last more than forty years (and was ended only by the break-up of the Soviet Union in 1991).

For some time, the US administration, influenced also by the emotion aroused in public opinion by the launching of the bombs on Japan, had directed its efforts towards international control of atomic energy, especially under the umbrella of the United Nations Organization (UN), which was founded at the San Francisco conference in April–June 1945. Here we shall not go into a discussion concerning the reasons for the launching of the two atomic bombs on Hiroshima and Nagasaki decided by President Truman: there has been much debate about the actual effectiveness of the bombing with regard to the final victory and ending the war, and about the possibility that the decision was rather a signal to the potential future enemy, the Soviet Union.

The consequence of the atomic explosions in Japan was a radical divergence of opinions in the scientific community, which had made such important contributions to the building of the atomic bomb. The danger of the Nazis winning the war had gathered together many scientists of different nationalities around the Los Alamos project. After the war, several of them, such as Bush and Oppenheimer, contributed actively to the formulation of a US nuclear policy; many others, like Einstein, Bohr, or Szilard, undertook a determined pacifist campaign against the further development of atomic weapons. A few months after the end of the war, the Federation of American Scientists (FAS) was set up with the main objective of opposing a further expansion of nuclear weaponry and disseminated its ideas through the *Bulletin of Atomic Scientists*. Von Neumann from the outset distanced himself from the FAS and avoided making any public statement concerning the military use of atomic energy.[90] He essentially reserved for himself a role of *expert*, that is, he placed his technical competence at the service of military and political decision-making. However, he did not shy from

89) See Zubok, Pleshakov 1996 and Zubok 2007, and the list of sources and studies published in Russia since the end of the Soviet Union on the origins of the Cold War contained in Zubok 2007.

90) An analysis of the 1948 exchange of letters between von Neumann and FAS members conserved in the Library of Congress may be found in the chapter "Von Neumann and the Arms Race" by Heims (1980, 235).

the new need for public commitment by scientists in the atomic age. His attitude, expressed through various consultative bodies as well as in his actions, mainly in narrow government circles or in academe, left no doubt as to his support for the further development of nuclear technology as a guarantee of national security and the key to the United States' international role in the defense of economic freedom and democracy.[91]

His patriotic attachment to the United States, which is quite understandable in view of the previous events in his life, was added to his hostility towards the Soviet Union: once Hitler's Nazism had been defeated, the Soviet Union became the new totalitarian enemy in the face of which no hesitation was possible. In these circumstances, von Neumann's profile again came to the fore: his interest in public activities and high level relations and his capacity not only to give scientific and technical advice but also to collaborate with the executive machine sharing a general political framework. In particular, he displayed a great naturalness in his relations with the military, something that was not always found easy by other scientists. In 1947 he was awarded the US Presidential Medal for Merit, as well as the US Navy Distinguished Civilian Service Award. This put the seal on a solid pact and von Neumann's position as a trusted expert of military and government bodies was further consolidated.

In 1946 the Atomic Energy Act was approved which established the Atomic Energy Commission (AEC), a body that had a strong influence on American military policy in the postwar period. In the meantime, research continued at Los Alamos within the framework of a more select working group including von Neumann, Teller, and Ulam. In June 1946 the Laboratory produced a very optimistic secret report concerning the prospects of the new project: the construction of a new more powerful bomb based on nuclear fusion, the so-called H bomb or thermonuclear bomb. In July 1946 von Neumann participated as an official observer in the atomic tests carried out at Bikini Atoll in the Marshall Islands in the South Pacific. Work continued in secret for several years while the cold war climate between the United States and the Soviet Union increasingly dominated the international political situation.

Communism spread rapidly to the eastern European countries, with the resulting formation of a strong Soviet Union area of influence. In 1948, a blockade of West Berlin (the partition of Berlin was part of the Yalta accords) led to the famous US Berlin airlift. In February 1949 the communist revolution was victorious in China and the US atomic monopoly was ended on August 29, the symbolic date of the explosion of the first Soviet atomic bomb in the Kazakhstan Republic. In June 1950, North Korean troops invaded the South, causing the US to intervene under the UN flag. All efforts to exert control over arms were abandoned and the priority aim for both superpowers became that of gaining military superiority and of preparing for possible conflict.

President Truman then definitively took the decision to develop the new superbomb, an idea pleaded by one influential AEC commissioner, Lewis Strauss, and

91) One fundamental piece of evidence is a draft of testimony "Nomination of John von Neumann to be a member of the United States Atomic Energy Commission", 8 March 1955 (see above section 4.3 "Scientific commitment during World War II").

supported by the Secretary of State John Foster Dulles. The AEC General Advisory Committee, chaired by Oppenheimer, was instead against it, defending the idea of producing small tactical nuclear warheads and recommending there should be a public debate on US nuclear policy. The Committee succeeded in convincing the majority of the AEC members, but President Truman nevertheless decided to go ahead with the project. This was a great triumph for von Neumann and Teller, who were convinced champions of this decision. As Heims has shown, the identity of views between the powerful Strauss and von Neumann led to a strong alliance being forged between the two and von Neumann, with Strauss's support, began to gain entry to several influential bodies. Starting from 1950, he actually began to take part in the meetings of various committees in Washington, such as the Weapons Systems Evaluation Group, the Armed Forces Special Weapons Project and, after 1951, the Air Force Scientific Advisory Board. The H bomb research culminated in the first test, known as Mike Shot, made in the Marshall Islands on 1 November 1952: the small island of Eniwetok was wiped off the map. In the same year, von Neumann filled a vacancy on the AEC General Advisory Committee, while Oppenheimer, when his term of office expired, was not re-elected chairman and rapidly lost influence.

The problem of the United States retaining its supremacy led to wide-ranging debate in government circles concerning the orientation of military policy. The idea then arose – apparently supported also by von Neumann – of a pre-emptive strike, involving a nuclear attack directed towards strategic points, aimed at defeating the enemy before it became too strong. Although this idea came to nothing, the study of possible scenarios in a future conflict underwent an extraordinary expansion. Von Neumann's game theory was applied extensively to these problems, in particular to the strategic analyses developed for the US Air Force by the RAND Corporation, an institution with which he collaborated systematically from 1948 on.

In those years, US foreign policy was dominated by the military problem and the atmosphere inside the country was laden with tensions. At the end of 1952 General Eisenhower, a World War II hero, was elected president: under his presidency, Strauss's influence increased even further. Eisenhower had given assurances that he intended to limit arms growth: this paradoxically was translated into a decision to reduce conventional weapons in favour of atomic arms, apparently in obedience to the principle of achieving greater power at a lower cost. Indeed US foreign policy in the 1950s continued to be based on atomic deterrence as a diplomatic weapon for containing the Soviet plans for territorial expansion. Eisenhower himself, in his speech to the nation in 1961, explicitly referred *a posteriori* to the reflection of this policy on the country administration: the enormous influence exerted on it by the industrial-military complex and the rise of the leadership of a scientific-technical *élite*. Von Neumann was without doubt one of the most distinguished members of this *élite*.

According to Heims, as a member of the AEC General Advisory Committee – and since February 1953 chairman of the weapons sub-committee – von Neumann advocated an unrestricted development of nuclear technology: the production of increasingly powerful bombs, the need to carry out atomic tests, and perfection of the delivery systems.[92] The latter aspect was the responsibility of the Air Force (AF),

on whose Scientific Advisory Board von Neumann sat: technological development in this field produced a great quantum leap consisting in the transition from bombers capable of launching bombs on the target to guided missiles. In 1953, von Neumann became chairman of the Nuclear Weapons Panel, set up as part of the above-mentioned AF scientific board as well as chairman of an *ad hoc* committee of the AF, the Strategic Missiles Evaluation Group, known as the "von Neumann committee". A further promotion of von Neumann into the spheres of power occurred, again in 1954, when Donald Quarles, the assistant secretary of defense for research and development, appointed him to the Technical Advisory Panel on Atomic Energy of the Department of Defense. However, he reached the apex of his career in 1955, when he was appointed a member of the AEC – the highest official position a scientist could reach in the US government – thanks to the firm support of Strauss, who chaired the Commission at the time.

4.5 Scientific research and national security during the Cold War

Backed by his strong determination, von Neumann had persisted, without any apparent hesitation, through the great conflicts caused by the rearmament race of the 1950s. After Stalin's death in 1953 and Eisenhower's election to the US Presidency, a few timid attempts to reach disarmament agreements were made in UN circles, although the hopes they raised were soon dashed. The tension did not abate and the technological-military race continued. In August 1953 the Soviets tested their first thermonuclear bomb. Oppenheimer, in his capacity of chairman of a consultative committee on disarmament set up by Eisenhower in 1953, invited the President to make the armaments debate public. The climate created by the anti-communist campaign of the Wisconsin Senator Joseph McCarthy caused a scandal that shook the US scientific community and the institutions concerned with nuclear policy: Oppenheimer was declared a danger to the country's security by the AEC because of alleged attempts to hinder the H bomb program and an alleged conspiracy with well-known communists in the 1930s. A commission of inquiry cleared Oppenheimer of these charges in June 1954 after a long and difficult hearing. Although these events aroused strong echoes in public opinion, even greater repercussions came from the worries regarding nuclear fallout, such as the radioactive rain on Nevada between 1953 and 1955 and the incident during the "Operation Castle" tests in the South Pacific in March 1954.

The Oppenheimer case shook von Neumann who, in his declaration before the commission of inquiry, unlike Teller, fully supported his colleague, with whom he had long-standing bonds of scientific collaboration. It should be pointed out in this connection that Oppenheimer had also been director of the IAS since 1947 and, in

92) The line of action followed by von Neumann deserves further study insofar as classified sources will be available for historical research.

this capacity, had supported von Neumann's project to build a scientific research calculator at the Institute – the Electronic Computer Project, ECP. However, neither this nor other events caused von Neumann to deviate from his strenuous opposition to any policy of disarmament or even of control over atomic experiments. There is no doubt that this attitude, like that of his friends and colleagues who had had similar experiences to him, was strongly influenced by the memory of the disastrous consequences of the pacifist policy of disarmament of the western powers in the face of the rise of Nazism.[93] Nor does he seem to have had any doubts about the legitimacy of the actions of the AEC and other military organizations, which were actually acting outside the sphere of public opinion control. Suffice it to consider that the half-yearly reports of the AEC referred only to civilian operations, with only very brief outlines of the armaments programs that were indeed the very focus of its activities (Heims 1980, 280–281). Information on the consequences of the atomic tests on ecological equilibrium and health in the areas used to carry them out reached American public opinion only in the final decades of the century.

On these topics it is interesting to read several articles published by von Neumann between 1954 and 1955. In a lecture on the "Impact of atomic energy on the physical and chemical sciences" delivered at the Massachusetts Institute of Technology in 1955, he spoke of the new responsibilities of the scientist in the atomic era and with foresight he predicted that developments in disciplines other than the physical sciences would in future raise social problems of no less import – as indeed has happened in the field of biology and biotechnology:

> *Scientists who made the control and release of atomic energy possible were among the first to feel the changes which this astounding development entailed. They are no longer free to carry on their research in isolated "ivory towers" completely free from the need for accounting for the possible uses of their discoveries. They are a decisive part of our atomic age civilization.* [...]
>
> *Pure science is often abstruse, and yet scientists may today be called upon to fill positions of considerable responsibility in fields outside their professional area of competence. They may become administrators, they may have to influence public opinion; all in all, they have great social responsibilities. We must expect that other phases of abstract thinking, other than physics and chemistry, may also ultimately evolve into similar roles; that is, they may assume military, economic, and more generally social roles*

93) In a letter to Veblen of October 1938, von Neumann wrote: «As to "great" politics I can only say, that Mr. Chamberlain obviously wanted to do me a personal favor, since I needed a postponement of the next world war very badly. Otherwise: The liquidation of the Tchecoslovak affair may not be unreasonable in itself, possibly Tchecoslovakia will get now those frontiers which she ought to have had in the first place, after 1918. But I can't see any reason, why European politics should not be in the same mess 6 month's hence, as they were 6 month's ago. At any rate I feel closer to your views about the British conservatives, than before». (Neumann (von) to Veblen, October 3, 1938, O. Veblen Papers, Library of Congress, now in Rédei (ed.) 2005, 271)

> *of equally tempting, compelling, and dangerous aspect. Science and scientists have become affected with the public interest in a new way and in orders of magnitude that were never imagined a half century ago.* [. . .]
>
> *We must recognize that the education of the scientist of the future is not complete as long as it is limited to his technical professional subjects; he must know something of history, law, economics, government, and public opinion. Our task is to make the adjustment to the new conditions as satisfactory as possible. We must do this intelligently and promptly. But we must do it without endangering the foundations upon which the sciences themselves rest and thrive.*[94]

In this way he pointed to a new path for scientists to follow. They must understand the need for a new role for them in that, as a "decisive part" of contemporary society, they were called upon to engage in a decisive intervention in the process followed by society in making the choices most convenient for it. Such a role could be played by a high-level *élite*, capable of rationally judging and deciding the best policy for the country. The role of science and scientists – according to the model he himself embodied – was thus not restricted to the development of technology and industry alone, but was extended also to the political and social decision-making processes. In this sense, his point of view, albeit in such a different historical context, put forward the late seventeenth century Enlightenment ideal of a society guided by scientific rationalism.

Von Neumann entrusted the lucidity brought by strategic analysis based on his idea of *strategic game* and relied definitely on the predominance of logical analysis. These views – together with a strong loyalty towards his new country – seem to be the sole basis of his political outlook in the short article on "Defense in Atomic War" written after his nomination as AEC commissioner for a Symposium in honor of his colleague Kent of the Aberdeen Laboratory:

> *The difficulty with atomic weapons, and especially with missile-carried atomic weapons, will be that they can decide a war, and do a good deal more in terms of destruction, in less than a month or two weeks. Consequently, the nature of technical surprise will be different from what it was before.*
>
> *It will not be sufficient to know that the enemy has only fifty possible tricks and that you can counter every one of them, but one must also invent some system of being able to counter them practically at the instant they occur.*
>
> *It is not easy to guess how this is going to be done. Some of the traditional aspects of the use of the same weapon for several purposes and of limiting*

94) Neumann (von) 1955c, in JNCW, vol. 6, 522. Once again we emphasize that these phrases illustrate von Neumann's change of attitude towards an interest in applied science and the conviction he had acquired that science could no longer keep its distance from its practical applications. But this has nothing to do with the alleged changes in his scientific conceptions.

its use until you need it for defense may have some of the elements of an answer.

However, this will probably mean that you will be forced not to 'do your worst' at all times, because then when the enemy does his worst you cannot defend against it, and the one thing you can put on at an instant's notice, if you are strong enough, is power stepped up to the limits of your capabilities. Hence, you may have to hold this trump card in reserve.

Without going further into the details of this matter I just wish to indicate generally that quite unconventional methods of systems analysis and of operations analysis will bear fruit in the future, as they did in the past.[95]

This passage is extremely enlightening, as it illustrates the well-known conception of the strategic balance of terror intended to avoid total destruction. It is not difficult to perceive in the theoretical justification of this conception given by von Neumann an echo of the pessimistic and moderate realism informing his view of game theory and that underlies the minimax solution: not to seek to obtain the maximum possible (in this case, "to do worse") otherwise one runs the risk of obtaining the least appreciated result ("we will not be able to defend ourselves"). In the case of nuclear war the most reasonable choice is to display a power boosted to the possible maximum in order to deter the enemy from taking action.[96] The analogies with views based on von Neumann's game theory are quite clear. Moreover it is a fact that game theory was one of the core topics in the war strategy think-tank represented by the RAND Corporation. However, it is not legitimate to go too much further and try and establish close analogies between von Neumann's strategic thinking and game theory theorems.[97]

95) Neumann (von) 1955d, in JNCW, vol. 6, 525.

96) As this passage clearly shows if it is true – as indeed it seems – that von Neumann was a champion of a "first strike" on the Soviet Union, it is not possible to relate this proposal to a general theoretical view which, at least from here, seems to be in favour of a different or at least more problematic view. If anything, the "first strike" could be interpreted as a warning shot.

97) This is the main shortcoming of William Poundstone's book on von Neumann (Poundstone 1992). Poundstone endeavours to establish a link between the nuclear policy trends and the so-called "prisoner's dilemma". In fact, as Poundstone himself admits, this famous example was conceived of for purposes quite alien to military problems, that is to jeopardize Nash's concept of equilibrium (on this topic, to which we shall come back later, see also Nasar 1998). Only later was it attempted to find a link between the content of this case and the nuclear problem, with uncertain and quite controversial outcome. However, von Neumann was extraneous to and disinterested in this type of problem, also because of his fundamental dissent with Nash's approach in game theory (also this point will be discussed later). For this reason Poundstone's book does not succeed in establishing any reasonable connection. Its treatment is a set of disjointed parts that could fill three separate books: the strategic questions, game theory, and the life of von Neumann. Moreover, von Neumann's biography is the acme of gossip: the author collects all the possible and imaginable anecdotes, even the most improbable, and does not stop short in the face of a detailed exhibition of the more minute aspects of von Neumann's physical and mental breakdown on his death bed. Even von Neumann's intelligence is rendered in a comic fashion: it is reduced to that of a kind of "idiot savant", a monstrous memory, notable above all for retaining anything, even list of names from a telephone book.

Figure 4.1 Stone plaque in front of the Budapest house of the Neumann family (62 Bajcsy-Zsilinszky street)
Courtesy of the Budapest University of Technology and Economics (BME), Institute of Mathematics

4.6 Freedom and limitations in the development of science and technology

The solid scientific and political outlook that von Neumann developed in response to the new conditions of the activity of the twentieth century scientist explains the way in which he succeeded in measuring himself with the research carried out in the military and industrial field. On the one hand, there was the need to carry on research covered by secrecy and by patents, and forego the publication of the results and their dissemination in the international scientific community; on the other hand, the limitation of research to technology-oriented programs meant an invasion of the holy ground of scientific freedom by a utilitarian conception. Von Neumann was always extremely sensitive to this type of implication. Consequently, at the end of World War II, his collaboration on the EDVAC calculator project led to a controversy with the chief engineer of the project at the Moore School of the University of Pennsylvania, J. Presper Eckert, and with the group's mathematician, John W. Mauchly. In Spring 1945 von Neumann had written a report containing the discussions concerning the new calculator ("First draft of a report on the EDVAC")[98], which was distributed in June to the group and the government services funding the project. The report, about 100 pages in length, was circulated informally among various US and British groups even though it had not actually been published, providing a good example of post-war scientific "gray literature". This report, which was much appreciated, became the starting point of a controversy, not only over intellectual priority of the key ideas for EDVAC, but also legal rights for industrial and commercial exploitation of the ideas.

In March 1946, Eckert and Mauchly were compelled to leave the University of Pennsylvania as they had refused to sign a new agreement proposed by the university according to which the rights of all the inventions of its employees automatically belonged to it. They later became partners in a company set up to commercially exploit their work and, at that stage, the Pentagon lawyers intervened to resolve the question. On the strength of the above-mentioned report, von Neumann defended his right to patent the idea on which the EDVAC was based: the result was that these ideas were considered as belonging to the public domain without any of the rival parties having the right to patent them. In the meantime, von Neumann changed the patenting policy of the ECP, which was then in full development inside the IAS, by abolishing the rule that the rights belonged to the engineers who had been the individual authors of the contributions and considering all the ideas as linked to the public project; he also excluded the Eckert and Mauchly group from all access to information concerning the project. In May 1946 he wrote in a private communication concerning the "EDVAC affair": «I would never have undertaken my consulting work at the University had I realized that I was essentially giving consulting services to a commercial group».[99] Even though he collaborated with several private companies in the role of consultant, he always rigorously upheld the distinction between industrial research,

98) Neumann (von) 1945a. On this matter see Aspray 1990.

99) Neumann (von) to Aaron Townshend, June 6, 1946, quoted in Aspray 1990, 45.

academic research and that performed on behalf of the government. Indeed his activity was always that of an expert; only when his term of office as a member of the AEC was about to expire did he begin to make plans for an entrepreneurial activity – thus returning to the commercial origins of his family – although this never actually came about.

This matter provides an occasion to examine in greater detail a central issue that could not fail to emerge in all its complexity to a scientist who had decided to plunge into the minefield of the social, political, military and industrial use of science, with all the ethical problems this entailed: the problem of the relations between scientific research and the constraints on its freedom, as a function of the risk associated with its applications.

Consistent with the attitude he had shown in the "EDVAC affair", he took a dim view of all purely utilitarian conceptions of science. In the notes he wrote for his statement before the US Special Senate Committee on Atomic Energy of 31 January 1946 (Neumann (von) 1946a, available in JNCW, vol. 6), he issued a very concrete warning against possible errors in the field of action by government agencies (in particular, the military) regarding the direction of scientific research and the planning of national science policy. The warning was based on a genuinely classical conception of science:

> *In regulating science, it is important to realize that the legislator is touching at a matter of extreme delicacy. Strict regulation, and even the threat or the anticipation of strict regulation, is perfectly able to stop the progress of science in the country where it occurs. The fact that strict or unreasonable regulations may deter mature scientists from pursuing their vocation, or from pursuing it with that degree of enthusiasm which is necessary for success, is in itself important, but it is not the most important fact. What is more fundamental is this: The numbers of new talent which accede in any one year to a given field of science are subject to considerable oscillations. They decrease or increase in response to the emergence of new interests, to changing social valuations, to new developments in the field in question, or in neighboring, scientific or applied fields, etc. I am convinced that seemingly small mistakes in "regulating" science may affect the "reproduction" of scientists catastrophically.*
>
> *Thus, erroneous legislation on this subject may harm science in this country seriously, even irremediably. Great intellectual values could be lost in this manner. Apart from this, damage to fundamental science would, at the present stage of industrial development, soon cause comparable damage in the technological, and then in the economical sphere. Finally, since other countries may not be similarly affected, it would seriously impair the national defense.*
>
> *For all these reasons I am absolutely convinced that it is necessary to maintain and to protect the natural* modus operandi *of fundamental research, and specifically two of its cornerstones: freedom in selecting the*

> *subject of fundamental research, and freedom in publishing its results. Any attempt to subdivide nuclear physics is futile from the start. To make work on the fission of heavy nuclei a preserve for special rules or for secrecy would be vain, since the reactions of light nuclei may later assume an even greater importance. To police all work on transmutations of all atomic species may still prove inadequate: Still other sources of primary energy may exist in processes yet to be discovered. Science, and particularly physics, forms an indivisible unit, and no attempt to compartmentalize it can produce anything but disappointment. And to put all of atomic physics, or all of physics, on the restricted and classified list would clearly kill the physical sciences.*[100]

However, it was no longer possible to turn a blind eye to the hazards science had produced for society as a whole and even for the environment:

> *It is for the first time that science has produced results which require an immediate intervention of organized society, of the government. Of course science has produced many results before which were of great importance to society, directly or indirectly. And there have been before scientific processes which required some minor policing measures of the government. But it is for the first time that a vast area of research, right in the central part of the physical sciences, impinges on a broad front on the vital zone of society, and clearly requires rapid and general regulation. It is now that physical science has become 'important' in that painful and dangerous sense which causes the state to intervene.*
>
> *Considering the vastness of the ultimate objectives of science, it has been clear for a long time to thoughtful persons that this moment must come, sooner or later. We know that it has come.* [...]
>
> *Regulation is needed, because nuclear physics, in combination with irresponsible or clumsy politics, could at this very moment inflict terrible wounds on society. And with some more development, which could be effected – and probably will be effected by some country or other – in a moderate number of years, and the main outlines of which are perfectly discernible today to the expert, the same combination of physics and politics could render the surface of the earth uninhabitable.* [...] *Regulation of science, on the other hand, must not go too far, Indeed, it should not go very far in any event, no matter how great risks are involved.*[101]

The two passages show up all the complexity of the problem and the contradiction that is opened: on the one hand, regulation is *necessary*, on the other, it *must not go too far*, without him clearly indicating the level at which it should be halted. However,

100) Neumann (von) 1946a, in JNCW, vol. 6, 500–501.

101) Ibid., 499–500.

the logical sequence with which the two horns of the dilemma are presented – in the text the passages occupy a position that is the reverse of the way we presented them – provides a clear indication of his inclination. It is no longer possible to deny the need for some form of regulation and the fact that «science has outgrown the age of independence from society» – a fact that «many scientists regret, and I am one of them».[102] But at the same time, everything possible should be done to preserve the freedom of science, and above all of "pure" or "basic" research, lest science itself be killed, and in perspective also technological and economic development:

> *There must, however, be no restriction in principle in any part of science, and none in nuclear physics in particular, and absolutely no secrecy or possibility of classification of the results of fundamental research.*[103]

It should be noted that this prospected balance proved extremely difficult to pursue, particularly in the field of military research. Indeed, in complete contradiction with the clarity of the statement that has just been quoted, much of his research was kept secret owing to its military implications, even when it had the status of fundamental research. In order to understand the difficulties involved in reaching this necessary equilibrium one should take into account that von Neumann's defense of research in the military field and his refusal of pure utilitarianism were part of a general assertion of the "rights of science" based on a radically *laissez-faire* conception.

Von Neumann actually defended the legitimacy of the risks of nuclear testing even after the "Operation Castle" accident, and was opposed to the drafting of a report for the United Nations on the risks of radioactive rain, on the grounds that it was contrary to United States' interests. It is worth making a short remark on this point. This kind of position is typical of the majority of scientists, not only during the postwar period, but also today. In the 1980s and 1990s a strong feeling emerged in many countries that it was necessary to exert control over technological development not only in physics but also in the biological field: this need is the subject of heated debate today. Nevertheless, it may be claimed without fear of contradiction that most research continues to be carried out without any problem, overcoming all obstacles, despite the activities of the various bioethical committees, which actually serve only to tranquilize public opinion and to isolate the problem, placing it back in the hands of the "experts".

The complex problems involved in the social use of science as well as the relations between pure and applied science were addressed by von Neumann on other occasions. A 1955 article in *Fortune* magazine with the significant title "Can we survive technology" is of particular importance (Neumann (von) 1955b). The half title preceding it expresses all of von Neumann's cautious realism and his lack of confidence in the innate nature of human rationality: «From the kind of explosiveness that man will be able to contrive by 1980, the globe is dangerously small, its political units dangerously unstable».

---

102) Ibid., 500.

103) Ibid., 501.

In this article he fully resumes all the topics he had committed ten years before to the consideration of the Senate Committee on Atomic Energy (Neumann (von) 1946a), as well as the problem of the initiatives that could or should be taken in order to prevent the risks inherent in science and technology. He traces out a very gloomy picture of these risks, not only as far as the nuclear issue is concerned, but also regarding climatic and environmental questions, revealing himself to be a clear-sighted prophet. However, it is precisely the global and catastrophic nature of the risk that prevents the prospects of a complete and satisfactory solution being taken too seriously:

> *Present awful possibilities of nuclear warfare may give way to others even more awful. After global climate control becomes possible, perhaps all our present involvements will seem simple. We should not deceive ourselves: once such possibilities become actual, they will be exploited. It will, therefore, be necessary to develop suitable new political forms and procedures. All experience shows that even smaller technological changes than those now on the cards profoundly transform political and social relationships. Experience also shows that these transformations are not a priori predictable and that most contemporary "first guesses" concerning them are wrong. For all these reasons, one should take neither present difficulties nor presently proposed reforms too seriously.*[104]

The reason why both the difficulties and the solutions should not be taken too seriously depends on the fact that the risk is not something that comes from the outside, but is inherent in the very structure of science and technology:

> *What kind of action does this situation call for?* Whatever *one feels inclined to do, one decisive trait must be considered: the very techniques that create the dangers and the instabilities are in themselves useful, or closely related to the useful. In fact, the more useful they could be, the more unstabilizing their effects can also be. It is not a particular perverse destructiveness of one particular invention that creates danger. Technological power, technological efficiency as such, is an ambivalent achievement. Its danger is intrinsic.*[105]

For this reason, however disturbing the panorama that lies before us, the only form of regulation is finally that of protecting the natural *modus operandi* of science, so that von Neumann displays a perfect continuity with his ideas of ten years before. One paragraph in the article has the significant title: "Science the indivisible". And here we find one of the reasons why, after unreservedly stating the risks involved in technology and science, he ends up by practically excluding any form of regulation:

104) Neumann (von) 1955b, in JNCW, vol. 6, 519.

105) Ibid., 515.

In looking for a solution, it is well to exclude one pseudosolution at the start. The crisis will not be resolved by inhibiting this or that apparently particularly obnoxious form of technology. For one thing, the parts of technology, as well as of the underlying sciences, are so intertwined that in the long run nothing less than a total elimination of all technological progress would suffice for inhibition. Also, on a more pedestrian and immediate basis, useful and harmful techniques lie everywhere so close together that it is never possible to separate the lions from the lambs.[106]

The second reason is that a restrictive and inhibitory attitude would be contrary to industrial era *ethos*:

Finally and, I believe, most importantly, prohibition of technology (invention and development, which are hardly separable from underlying scientific inquiry), is contrary to the whole ethos of the industrial age. It is irreconcilable with a major mode of intellectuality as our age understands it. It is hard to imagine such a restraint successfully imposed in our civilization. Only if those disasters that we fear had already occurred, only if humanity were already completely disillusioned about technological civilization, could such a step be taken. But not even the disasters of recent wars have produced that degree of disillusionment.[107]

So what is the realistic and possible answer? It must be based on the simplest and most direct pragmatism: to rely on a kind of "do it yourself" set of remedies, to trust in a form of innate capacity of mankind to survive disasters:

What safeguard remains? Apparently only day-to-day – or perhaps year-to-year – opportunistic measures, a long sequence of small, correct decisions. And this is not surprising. After all, the crisis is due to the rapidity of progress, to the probable further acceleration thereof, and to the reaching of certain critical relationships. Specifically, the effects that we are now beginning to produce are of the same order of magnitude as that of "the great globe itself". Indeed, they affect the earth as an entity. Hence further acceleration can no longer be absorbed as in the past by an extension of the area of operations. Under present conditions it is unreasonable to expect a novel cure-all.

For progress there is no cure. Any attempt to find automatically safe channels for the present explosive variety of progress must lead to frustration. The only safety possible is relative, and it lies in an intelligent exercise of day-to-day judgment. [...]

The one solid fact is that the difficulties are due to an evolution that, while useful and constructive, is also dangerous. Can we produce the required

106) Ibid., 515–516.

107) Ibid., 516–517.

> *adjustments with the necessary speed? The most hopeful answer is that the human species has been subjected to similar tests before and seems to have a congenital ability to come through, after varying amounts of trouble. To ask in advance for a complete recipe would be unreasonable. We can specify only the human qualities required: patience, flexibility, intelligence.*[108]

In actual fact, von Neumann's view cannot be reduced to simple pragmatism or to total *laissez faire*. The other decisive element in his view was the role of scientific knowledge and of scientists. Thus, in the article in question, it is not possible to overlook a section entitled "Rather fantastic effects". Although emphasizing the huge difficulty involved in predicting complex phenomena, such as the weather, here he optimistically claims that «our knowledge of the dynamics and the controlling processes in the atmosphere is rapidly approaching a level that would permit such prediction». He then goes on to say:

> *What would be harmful and what beneficial – and to which regions of the earth – is not immediately obvious. But there is little doubt that one* could *carry out analyses needed to predict results, intervene on any desired scale, and ultimately achieve rather fantastic effects.*[109]

Here there emerges a characteristic aspect of his thought: the disenchanted and skeptical realism concerning the capacity of human society to make the most rational and effective choices as a function of their well-being, is compensated by a very strong faith, an almost unlimited optimism concerning the opportunities offered by science, and in particular, by science founded on the mathematical method. So, the acknowledgement of the increasing complexity of the problems faced by mankind is tempered by confidence in science's growing capacity to predict and to provide the means required for intervention. Von Neumann always displayed an inclination to interpret complexity in terms of "complication", that is, as an obstacle that can be overcome by means of an increasingly sophisticated approach – more and more adequate models and of more and more perfected calculus procedures. This optimism proved to be excessive, in view of the subsequent developments regarding complexity and of the emergence of epistemological obstacles such as those raised by deterministic chaos theory, starting in the 1970s.

Von Neumann's choices were always determined by convictions rooted in a well-defined ethical view, and in particular in the primary value of democracy and free market ("the institutions of America", using his own words), which he considered as something to be defended at all costs. It is equally certain that science and the scientific view of things was the centre of gravity of all his thinking. This accounts for his deep-seated conviction that all questions may be resolved or prepared for a resolution that is at least reasonable and acceptable provided it is addressed in accordance

108) Ibid., 518–519.

109) Ibid., 513.

with the principles of scientific rationality; and this accounts for the central role he attributed to the scientific community as the enlightened guide of society: only scientists could guarantee a management of society as rational as possible. We have above described this as a neo-enlightened point of view because it revived eighteenth century scientific philosophy principles – albeit in a distant and quite different context. Also this can provide some explanation for his enthusiastic endorsement of the ideals of American society, which he perceived as being firmly rooted in those principles.

Needless to say this view contained some illusory aspects. Not only because it was based on faith in the constant increase in science's predictive capacity, which ultimately proved to be somewhat over-optimistic. But above all because – on the social plane – the accordance to the learned of a dominant role in government, although in deference to criteria of rationality, contains the germ of tyranny.[110] In actual fact, the growing weight that accrues with a scientific background like von Neumann's in the US government, in close coordination with military and industrial leaders, led to the formation of a scientific and technological elite, which ultimately exerted unbounded power, capable not only of challenging the spheres of political power, but of actually influencing its decisions and of straining the traditional rules of democracy. It has already been mentioned how the most sensational admission regarding this state of affairs was made by President Eisenhower himself in a speech delivered on 17 January 1961 in which, taking stock of his government's activity, he revealed the conflicts and problems he had had to face and presented a lucid analysis of the evolution of scientific and technical endeavour:

> *In the councils of government, we must guard against the acquisition of unwarranted influence, whether sought or unsought, by the military industrial complex. The potential for the disastrous rise of misplaced power exists and will persist.*
>
> *We must never let the weight of this combination endanger our liberties or democratic processes. We should take nothing for granted. Only an alert and knowledgeable citizenry can compel the proper meshing of the huge industrial and military machinery of defense with our peaceful methods and goals, so that security and liberty may prosper together.*
>
> *Akin to, and largely responsible for the sweeping changes in our industrial-military posture, has been the technological revolution during recent decades. In this revolution, research has become central; it also becomes more formalized, complex, and costly. A steadily increasing share is conducted for, by, or at the direction of, the Federal government.*

110) It is worth recalling in this connection the brilliant analysis made by Alexandre Koyré: «Politics reminds us that only knowledge justifies the possession and exercise of power and that, consequently, the ideal statesman, invested with absolute power, with a power that is not governed and circumscribed by law, may exert it justly only if he is endowed at the same time with absolute knowledge. There is no doubt that a City managed and governed by a *politikós* possessing such a "science" [. . .] would be happier than a City governed by the law. [. . .] Alas, such a science is not human. The ideal *politikós* would have to be a wise man: even more, a god. If he were a man, that is, if a man placed himself above the law, he would necessarily be a tyrant» (Koyré 1962).

> *Today, the solitary inventor, tinkering in his shop, has been overshadowed by task forces of scientists in laboratories and testing fields. In the same fashion, the free university, historically the fountainhead of free ideas and scientific discovery, has experienced a revolution in the conduct of research. Partly because of the huge costs involved, a government contract becomes virtually a substitute for intellectual curiosity. For every old blackboard there are now hundreds of new electronic computers.*
>
> *The prospect of domination of the nation's scholars by Federal employment, project allocations, and the power of money is ever present and is gravely to be regarded.*
>
> *Yet, in holding scientific research and discovery in respect, as we should, we must also be alert to the equal and opposite danger that public policy could itself become the captive of a scientific technological elite. It is the task of statesmanship to mold, to balance, and to integrate these and other forces, new and old, within the principles of our democratic system – ever aiming toward the supreme goals of our free society.*[111]

This is authoritative testimony which provides some highly topical material for reflection concerning the risk of the idea that, although with the best of intentions, the technological and scientific apparatus is able on its own to provide all the elements required for a rational and "just" management of society.

4.7 Systems, information, control

Von Neumann's collaboration with the AOD in the field of the development of automatic calculation continued after the war, first in continuation of construction of the EDVAC calculator and then in other developments of information technology. He prepared a number of reports for the NBO and other military organizations, which were deemed to be of enormous importance. Nevertheless, as internal reports many of them were not published and were in some cases classified. They were still covered by secrecy when Abraham H. Taub was preparing an edition of the complete works of von Neumann, published between 1960 and 1963. For example, the hitherto unpublished material contained in it included a report written together with Goldstine *On the principles of large scale computing machines* (Neumann (von), Goldstine 1946), which gathers together materials from several lectures given around 1946, and a report written in 1949 to the ONR on *Recent theories of turbulence* (Neumann (von) 1949c).

As to non-military consulting, in Autumn 1946 he was appointed chairman of the NRC Committee on High-Speed Computing, and in 1948 he drew up reports for the National Bureau of Standards concerning the building of computers for various

111) Farewell Radio and Television Address to the American People by President Dwight D. Eisenhower, January 17, 1961. http://www.eisenhower.archives.gov/speeches/farewell

government agencies. Some of his efforts were also directed towards organization of the new field of computer science and technology. Thus the scientific journal *Mathematical Tables and Other Aids to Computation* – later renamed *Mathematics of Computation* – was reorganized so as to take into account the developments of electronic computers. Nevertheless, the idea of setting up a special scientific society was abandoned as von Neumann considered that the time was not ripe for it.

William Aspray has made an exhaustive analysis of von Neumann's fundamental contribution to the birth of computer science and information technology, with his theoretical contribution, his institutional activity and also the development of the IAS Electronic Computing Project (ECS). The ECS was essentially conceived of by von Neumann as providing support for scientific research through numerical analysis. Still, in the final years of his life, his theoretical computer science research merged with the logic research of his youth, and led him to rough out his last famous scientific contribution – the theory of automata. We shall examine these aspects in Chapter 5. What we should like to look at in this section is his contribution to the rise of the "systems approach" and the culture of control during the second half of the twentieth century.

Indeed, from a broader cultural view, the development of the computer in the postwar years, side by side with other technological achievements in the field of automatic control engineering, aeronautics, and so on, converged towards the creation of large-scale technical systems and networks. First linked to national security, these large-scale projects extended to the aerospace sector and civil sectors (from transportation networks, to urban planning, public administration, or industrial production and distribution systems) and cemented the alliance among science, technology and industry of the second half of the century. Von Neumann played an important role also in this evolution, as has been shown by recent studies on the emergence of the systems approach in the United States.[112] His contribution was the result of his collaboration with the Air Force, the military body which formed the basis of a new network of relations between scientists, engineers and the military world.

In the years of the Cold War, the nightmare scenarios opened up by the nuclear arms race loomed over the destiny of the entire planet. However, implementation of large technical systems perhaps left an even stronger mark on the cultural atmosphere of the time. In the eyes of his contemporaries, they represented a true "triumph of technology" and thus contributed to dissipating the fears aroused by the atomic bomb and the fears it projected over scientific and technological development. It lay at the basis of the uninterrupted confidence shown by public opinion in the advance of technology and above all in the possibility of maintaining control over this development.

112) On the topics treated in this section, see Hughes 1998, who, without losing sight of the political-cultural context of the Cold War, presents a historical analysis of these developments as linked to the evolution of the concepts of system, network, and control in the history of technology (see, for example, Hughes 1983) and affords a deeper understanding of von Neumann's role. Less convincing on the topic is Edwards 1996, who subordinates the scientific-technological thought of the postwar period to the influence of a command and control view borrowed from the military context.

These systems were characterized by continuity with the technological progress of preceding decades towards the creation of networks and integrated technical systems in the civil and industrial fields (communications, transport, energy production and distribution). However, as shown by Agatha and Thomas Hughes, in the period 1939–1960 an innovative vision of systems, the *systems approach*, emerged in military circles, which exploited the new technologies of automation, information, communication and mathematical programming:

> *Practitioners and proponents embrace a holistic vision. They focus on the interconnections among subsystems and components, taking special note of the interfaces among various parts. What is significant is that system builders include heterogeneous components, such as mechanical, electrical, and organizational parts, in a single system. Organizational parts might be managerial structures, such as military command, or political entities, such as government bureau. Organizational components not only interact with technical ones but often reflect their characteristics. For instance, a management organization for presiding over the development of an intercontinental missile system might be divided into divisions that mirror the parts of the missile being designed.* (Hughes, Hughes (eds.) 2000, 2).

This philosophy was adopted by the US Air Force which, in 1947, had become a separate service from the Army, and which lay at the centre of the development of newly conceived large-scale defense systems. The dominant interest of the AF for offensive aircraft, and consequently also of the aeronautic industry, was now accompanied by a fresh interest in developments in electronics technology. The so-called Air Defense Electronic Environments provided the immaterial fabric of information and control which became essential in problems of modern defense with the development of "intelligent" vehicles and weapons. AF initiatives and funding had considerable spin-off in terms of the expansion of US corporations active in the aeronautics sector (Boeing, Douglas Aircraft) and in the development of companies operating in electronics. However, the cultural influence of these initiatives and funding went far beyond the well-tested model of alliance between government and private industry linked to US government military contracts.

The experience acquired during World War II had increased awareness of the growing technological component of modern warfare among several military heads and, after the war, the AF consequently mobilized some of the greatest scientists of the time and many of the best engineers (academics, but also active in industry). Of crucial significance in this development was the researcher panel set up by General Henry Arnold under the chairmanship of von Kármán at the end of the war, which resulted in establishment of the influential AF Scientific Advisory Board. Many other researchers (mathematicians, physicists, engineers, but also economists and students of the human and social sciences) worked for a research centre in California, in Santa Monica, which had the aim of exploring from all points of view non-terrestrial inter-

continental warfare. With the name of RAND (Research and Development) Corporation, it was set up in 1946 at Arnold's initiative as a division of the Douglas Aircraft corporation and in 1948 it was transformed into a non-profit company financially supported by the AF.

Von Neumann was contacted by the director of mathematical research at RAND, John Williams, in late 1947 and, from 1948 until 1955, he enjoyed a consultancy contract with this institution that carried out civilian research for military purposes.[113] He was a reference figure during the crucial years in which the RAND cultural project was developing and certainly made a contribution to allowing its many complementary facets to emerge. Above all he had regular contact with the engineers and physicists working on technical devices (in the missile, aircraft, electronics and nuclear physics departments) and which accounted for an important part of the activity, thus lining up with other agencies and laboratories. Let us recall what Rees said: von Neumann played a key role in the circulation of scientific-technical information during years in which the flourishing of activities and initiatives encountered considerable rivalries and difficulties in coordinating the research, both that carried out in civil institutions, with the support of the various services, and that in military laboratories, which furthermore reflected the different military projects and outlooks of the Navy, Army and Air Force.

However, RAND's original contribution, which distinguishes it from other postwar laboratories and think tanks, lies in the other sections that flanked the one concerned with hardware and aimed at an analysis of warfare in all its modern forms and on the basis of a range of different disciplinary approaches. Right from its foundation the RAND corporation included a group dedicated to so-called "military worth", from which the three departments of economics, social sciences and mathematics originated. "Military worth" was taken to mean the type of investigation and mathematical assessment of weapons and military objectives and operations that had gradually emerged during the war within the framework of various contracts and activities coordinated by Weaver's AMP. The cultural model was the one set up by British scientists in the "operational research" groups: it was thus an outcome of the emergency facing the UK right from the first signs of the danger represented by the policies of the Nazi regime in Germany and above all during the dramatic period of the Battle of Britain. It consisted of a proposed development of a new technical or "applied" knowledge regarding the operational problems of technological-industrial warfare, starting from anti-aircraft defense, protection of military convoys, production and logistics programming and strategic bombing. In the United States, and at RAND in particular, this proposal was taken up and concretely implemented through the contribution not only of mathematical and economic approaches but also approaches that were characteristic of the non-formalized human sciences.

From the mathematical standpoint, the theoretical architrave supporting this research was precisely von Neumann's theory of games. The latter was now transferred from the general problem of functioning of the market economy or the analysis of

113) See Leonard 2004 and Hounshell 1997.

rational behaviour in board games to that of national defense and national strategy and, more generally and in more abstract terms, to any "complex" activity, that is, in which many components exhibit a web of interrelations that are very difficult to reduce to a simple description – in particular man and the artificial creatures he is trying to govern. Indeed, as we saw earlier, game theory provided a possible "rational way" of analyzing such problems and thus a support to those decisions that had previously been entrusted essentially to the leaders, and in a time of war to the top-ranking military chiefs. In the RAND Corporation, game theory was systematically explored in the context of air force problems. It was applied directly to specific and well-defined situations such as those regarding dogfights between fighter aircraft and between fighters and bombers and, in this context, work was done also on its empirical verification using the methods of experimental psychology. Furthermore, the language and the conceptual framework of game theory underpinned the more ambitious enterprise undertaken by RAND – the development of a "large-scale systems analysis" applied to questions such as strategic bombing; or else an "organization theory" potentially applicable to the problems of national security and beyond, a question that also Morgenstern worked on at RAND in 1951. Even though von Neumann had not continued to work specifically on game theory after his fundamental book in 1944 – which will be described in Chapter 5 – except for a few sporadic articles, he accompanied this very early phase of exploration at RAND of a new kind of scientific problems with lectures and discussions during his frequent visits to the headquarters, as well as with opinions delivered also by correspondence.

The results of these efforts were, to tell the truth, rather disappointing and led to a profound debate on the very foundations of the theory. This was true, on the one hand, of the research undertaken by Merrill Flood and others after his arrival at RAND in mid-1949 concerning the effective value of game theory in the analysis of rationality and decision-making, both through a comparison between von Neumann-Morgenstern's cooperative approach and John Nash's non-cooperative approach, and through the attempts at experimental verification carried out in the Systems Research Laboratory directed by John L. Kennedy in 1951–52. Also the "systems analyses" were subjected to severe criticism by the military authorities and from inside RAND itself. Nevertheless, von Neumann's game theory had perhaps a more lasting effect in the RAND environment in less ambitious forms, more closely linked to specific practical problems, that is, by encouraging the view that mathematical optimization was a suitable way of seeking to develop techniques for the mathematical programming of interdependent activities (planning, resources allocation, logistics, transport and so on). Indeed, the postwar period saw an astonishing boom in these techniques (linear programming, dynamic programming, network flow analysis). They became the heart of operations research, which thus became an actual discipline potentially applicable far beyond military problems alone to all kinds of industrial and management problems in general. Many RAND researchers in the mathematics and economics departments contributed to this development, in collaboration with other university researchers, and in particular with the Cowles Commission for Research in Economics, a research institute affiliated with the University of Chicago.

The RAND Corporation processed the experience of the first two world wars, contributing to the consolidation of a new military thinking in the AF. This thinking was based, first, on the new computer, aeronautical, missile and atomic technologies; it was also depending on the geographic situation of the United States separated by the ocean from all conceivable enemies; and lastly, it was marked by a new "virtual" war view – or, in any case, a concept of war as "viewed from above" – that represented an evolution of the AF concept in terms of strategic bombing. In the meantime, RAND researchers also worked on a theoretical view of large technical systems and of the interaction among the technological and organizational aspects in planning, development and operational phases. Von Neumann's influence was felt on both these aspects, which will be further discussed in Chapter 5. However, in the early 1950s his visits to RAND slackened. In those years, his itinerary kept him distant from general scientific discussions, while it opened up to him bodies in which the executive decisions were made and the great projects were actually laid out.

The lead was taken in these projects by an advanced technological structure designed to defend the national territory of the United States against air attack by long-range bombers, denoted as SAGE (Semiautomatic Ground Environment).[114] SAGE was a direct spin-off from the antiaircraft fire control systems developed in the United States during the war, on the one hand, and on the other from British operational research groups on ground defenses in the UK aimed at optimizing human and technical resources (aircraft, radar, telephones) spread over the territory, in operations centres, military airfields, and radar sites. Indeed, the problem was to create an integrated defense system based on radar, communications technology and servomechanisms, for this purpose developing new programmable electronic computers. In 1944, with the support of the US Navy, the Servomechanisms Laboratory was set up at MIT to develop a computer-analyser to be used as an air defense information processor. The laboratory, directed by Jay W. Forrester, developed Whirlwind, a digital computer specifically designed to handle real-time command and control problems in a wartime environment. This was a different kind of machine, compared with previous computers, such as the one developed at the IAS by von Neumann and Julian H. Bigelow, designed for scientific calculation: in fact the solution of control problems potentially opened up a much wider scope.

During the development of SAGE under the direction of the MIT Lincoln Laboratory – set up for this purpose in 1951 in Bedford, Massachusetts – the problems of "systems engineering" emerged for the first time. In the Lincoln Laboratory, work continued on the Whirlwind computer as well as in other sectors dedicated to radar and communications linked aspects. In 1953 the SAGE "system architecture" was defined: US territory was divided up into eight sectors, each of them under the control of a "combat centre", i.e., an operations centre that received (over the telephone lines) and processed (with the help of a Whirlwind type computer) information from 32 subsectors. Each of the latter was presided over by an operations centre equipped

114) Here we follow Hughes' convincing analysis of the conceptual transition from SAGE to Atlas-ICBM (Hughes 1998).

with a computer and its own territory was dotted with radar sites at various levels, including some on platforms located in coastal waters which transmitted data via radio microwaves. The system was to be integrated with two others: a temporary system on which Bell Laboratories were working, based on the system inherited from the wartime period, and a more specific system, known as the Distant Early Warning System, which was entrusted to Western Electric.

What in the eyes of the military commanders was an antiaircraft defense system was also, from the point of view of MIT engineers who had developed it, a real-time information processing and control system based on electronic technology. The cultural mobilization stirred up by the interests of the AF consequently had a strong influence on the development of twentieth century engineering, not only as far as progress made by individual technologies is concerned (aeronautics, communications, computer science, and so on), but also because it brought engineers up against a new kind of problem: integration and coordination of highly complex technical projects. Companies were set up to provide specific consultancy regarding systems planning and management, both at the level of non-profit companies like RAND, and of private companies like the Ramo-Wooldrige Corporation, to be illustrated below. "Systems engineering" developed mainly among engineers working in the electronics and communications sector.

Actually the SAGE planning was initially developed following the conventional engineering approach, that is, as a set of individual problems of planning and producing its components. When the point of transition from the Whirlwind computer phase to the design of prototypes aimed at industrial production was reached, the IBM company was involved and gained enormous benefits from this experience. Computer programming was entrusted first to RAND and then, based on the latter as model, the System Development Corporation was set up, which concerned itself also with training of staff for the direction of operations centres. In the years that followed, however, it became increasingly obvious that the necessary high quality of functioning required for the entire project, both during the planning and implementation phases and when it came into service, raised problems of such a complexity that it could not be left to common sense or decisions on a case by case basis. In addition to the aspects directly linked to information processing and those related to weapons control, on which the Lincoln Laboratory focused, were those related to the design and construction of operations centres, radar installations, the telephone network, aircraft, ground-air missiles and other weapons systems; and each of these was the responsibility of a contractor from among industrial corporations (including Western Electric, Bendix Radio, the Bell Laboratories and Boeing), which in turn had its own subcontractors.

The SAGE project, as Hughes has shown, made a huge contribution to the development of computer science (technology of the digital electronic computer and programming) and automation (replacement of man in the transmission and analysis of information and also of human action in the command of antiaircraft defense operations); while awareness of the existence of a basic management and organizational problem gradually emerged.[115] From this point of view, SAGE was a pilot experiment

and, in part, a failure: after years of elaboration, when its first operating sector was inaugurated in 1958, the real threat had become intercontinental ballistic missiles.

The first of the great American missile projects that marked the military and political equilibria of the central period of the Cold War was the Atlas intercontinental ballistic missile (Atlas ICBM). Von Neumann's role in the conception and design of the Atlas was fundamental. Together with Teller he enjoyed enormous prestige among the AF high command. Their reports to the AF Scientific Advisory Board led to a panel being set up in early 1953 under the direction of von Neumann to assess the effect of technological developments – which had allowed a considerable reduction in the weight of thermonuclear warheads – on the design and construction of aircraft, ballistic missiles and cruise missiles. The panel's recommendations were favourable to the development of ballistic missiles and ultimately led to creation of the Strategic Missiles Evaluation Committee. The work of this committee, which assessed also information provided by the secret service on developments in Soviet missile technology and the various alternative technological solutions, enabled the AF to overcome a longstanding hostility to missiles and to give top priority to development of an intercontinental ballistic missile – the "Atlas project".

Mention has already been made of von Neumann's role in the arms race. Here we would like to describe the strong cultural influence that, ever since its first report in February 1954, marked the Committee's work.[116] It was above all the critical technical aspects of the project that were examined and an explicit defense was made of the advisability of carrying out a study of weapons system "adequately based on fundamental science".[117] Even more innovative, if compared with the SAGE experience, was the appeal to pay greater attention to the organization problem, that is, to the management aspects essential to a successful outcome of the project and the need to consider this organization problem as a technological problem in its own right. It was also discussed whether it was worth founding for this purpose a university institution or the like, or else to form a public administration to perform this task: it was finally decided to turn to a private consultancy company. The choice fell on the Ramo-Wooldridge Corporation, set up by two members of the committee, the engineers Simon Ramo and Dean E. Wooldridge. Immediately after, in 1954, the committee decided to change its name to ICBM Scientific Advisory Committee, appointed von Neumann as Chairman and entrusted to Ramo-Wooldridge all the management aspects linked to the project – from the actual meetings of the von Neumann Committee to the global conception of the missile system project, from specifications of contracts with various subcontracting corporations, to general coordination and assessment of the project's progress. Von Neumann did his utmost to get the authorities to approve the project, acting in concert with Air Force General Bernard Schriever,

115) This fact uncovered many dysfunctions that, as Hughes wrote, partly account for its relative lack of success. Only in 1958 was the management and organization problem regarding SAGE finally addressed with the creation of a new corporation, again non- profit, the MITRE Corporation, to which many researchers who had worked in Forrester's group were transferred.

116) See the analysis by Hughes (1998, 82 ff) of the report.

117) Quoted in ibid., 89.

the programme administrator, together with the famous aviator Charles Lindbergh, a member of his Committee, and with Trevor Gardner, AF assistant for research and development. Von Neumann also worked as consultant to Ramo-Wooldridge and the problems of trajectory, component miniaturization, and the problems of heat resistance were studied under his supervision (Aspray 1990, 249–50).

The observations and recommendations of the von Neumann committee had a strong impact on developments in engineering. In fact, in this way the role of coordination and management, which had often been reserved to aeronautics corporations as holders of the principal contract on behalf of the AF, was transferred to companies operating in the electronics sector:

> *By recommending that Ramo-Wooldridge act as a system engineer, the committee was displacing engineers familiar with traditional aeronautical engineering practice with scientists and engineers grounded in the sciences and especially experienced in electronics and computers. To the committee, the airframe was merely a platform to carry complex, electronic guidance and fire control systems.* (Hughes 1998, 98)

A new theoretical approach to management problems was put forward, which included two fundamental elements. On the one hand, the starting point was a global approach to system design, engineering and management marked by the technological conception of "control". On the other, the emphasis was placed, for each individual problem, on identification of the underlying logical and mathematical structures, and so a method of addressing the concrete material reality was chosen which involved the use of an abstract theoretical screen. The distance between this epistemological model and that of previous engineering was emphasized in those years by speaking of a transition from the classical engineering of "machines" to "systems engineering". The engineer's "organization technologies" were a basic aspect of the "culture of control" which spread through the United States and industrialized societies during the Cold War years. Specifically, two important aspects of it contributed to shaping the new organization technologies: on the one hand, mathematical programming techniques and, on the other, techniques stemming from the transfer of engineering control models to management problems, such as in the case of inventory control.[118]

Of course, this transformation was the result of numerous individual and collective contributions, including those produced by the RAND Corporation, as well as of the actual evolution of technologies, in the United States and abroad. However, there is no doubt that the Atlas ICBM Scientific Advisory Committee, by laying out one of the largest military projects of the Cold War, played a significant role in this process. In this connection, it is sufficient to examine its composition, in the two phases described above, which may be deemed representative also of the technical-scientific elite of the West coast of the United States, in opposition to the MIT group involved in

118) See Klein J. 1999, 2001a, Hughes, Hughes (eds.) 2000, Mindell 2002, Lucertini, Millán Gasca, Nicolò 2004 and Levin 1999. Further bibliography including engineering sources can be found in Millán Gasca 2006.

the SAGE project, which instead represented the powerful northeast cultural area of the country. In addition to von Neumann and a group of physicists and engineers from the California Institute of Technology – generally viewed as the leading institution in supporting a scientific approach to the problems of technology – members included the RAND Chairman, Frank Collbohm, the MIT professor and engineer Jerome Wiesner (future MIT chancellor and President Kennedy's science adviser), and several engineers from private corporations, such as Ramo and Wooldridge themselves and also Heindrik W. Bode, from Bell Laboratories, the author of a modern communications engineering classic, *Network analysis and feedback amplifier design* (1945).

The starting point of the SAGE project was still a weak attempt to explore the abstract conception of system. But it had immediately addressed the development and design of "real" system components, while the system as a whole was still intuitively conceived of on the basis of images inherited from the use of radar during World War II. The starting point for the ICBM project was instead a strong demand for a scientific theoretical approach set out by researchers such as Bode and von Neumann, who had developed the viewing of material processes – respectively in communications and in the electronic computer – through flow diagrams and feedback circuits of information carrying pulses and signals. The committee's approach received the full support of the military head of the Atlas ICBM project, Bernard Schriever.[119] The first test firing of an Atlas missile was in 1958 – a 2500 mile launch – and the project culminated in 1962 in construction of an even more advanced missile, the Titan, which had also been proposed in 1955 by the von Neumann Committee.

The knowledge accumulated in the field of military projects, at RAND as well as in the development of SAGE and the various ICBM phases, was transferred – despite the constraints of military secrecy – to both government projects of a not strictly military nature (such as NASA's aerospace research, or the public administration) and to large scale projects of civil engineering and the industrial world. For example, in 1952 Merrill Flood moved to Columbia, where he directed the Institute for Research in Management and Industrial Production; Charles Hitch, director of the RAND Economics Department, was one of the principal advisors of Robert MacNamara's revolution in which new techniques for rationalizing decision-making and cost control were introduced; in 1969 Simon Ramo published *Cure for chaos: fresh solutions to social problems through the systems approach*. These are but a few examples of individual itineraries that formed part of a complex and ramified cultural process. From the height of his scientific authority, von Neumann accompanied the first few steps of this process which, starting in World War II, rightfully thrust the United States academic world, and in particular, its scientific and technological sectors, into the network of relations between industry and the government of the country.

This network actually already existed beforehand, as has been effectively shown by research carried out on the links between military contracts and the development

119) On the relations between the von Neumann committee and Schriever and implementation of the project in the Western Development Division of the Air Research and Development Command, see Hughes 2000, 93 ss. and the references therein.

of industrial production organization in the United States. The novelty, without doubt due to wartime conditions and the Cold War, was actually to have involved scientists and engineers, as happened in France in the aftermath of the French Revolution, also under conditions of emergency for national security. However, the French experience came to naught with the end of the revolutionary phase and the Napoleonic empire, while the new US military-industrial-university complex was rapidly consolidated and, in the second half of the twentieth century, became a model of development for industrialized countries. Both the process of engineering the Revolution (Alder 1999), and that of engineering the Cold War had stimulating and beneficial consequences for scientific and technological creativity (the "generation of knowledge", to use the words of David Hounshell). However, in both experiences the lurking dangers of a new form of authoritarianism based on a scientific-technical oligarchy raised their heads. The words of Eisenhower quoted earlier represent an early signal of the fact that awareness was growing in the country of the risks faced by this kind of social model – risks for human life, for the planet and for democracy. Still, in the last decades of the century, this critical attitude often turned into a unilateral view of this experience and of the main figures – of course including also von Neumann. The reduction of the many facets of this process to a single formula (e.g., "closed world") in no way facilitates the quest for in-depth understanding, as Hughes has lucidly pointed out:

> *A historian attempting to persuade a post-Vietnam war generation that the military-industrial-university complex has presided over a highly creative period since World War II faces a difficult challenge. A generation influenced by counterculture values of the* 1960*s has difficulty in seeing the military-industrial-university complex as other than politicized, bureaucratic, and indiscriminate in the use of public funds, and destructive of the physical environment. Proponents of the free-enterprise system also have difficulty in appreciating the role of government in promoting technological change. Many Americans simply cannot conceive of the military as deeply shaping their culture and their values.*
>
> *If, however, the historian focuses on the period from* 1950 *to* 1970*, when military-funded projects dominated the innovative technological and managerial scene, a different picture emerges. Unquestionably, the system builders took little heed of the degradation of the physical environment at the sites where their systems evolved, but they energetically and effectively countered the bureaucratic tendencies common to large projects and organizations; they held at bay political forces that would subvert technical rationality. Motivated by the conviction that they were responding to a national emergency, they single-mindedly and rationally dedicated the enormous funds at their disposal to providing national defense. Opportunistic use of military funds to sustain regional economic development and corporate welfare came later.*[120]

In this direction a more balanced interpretation of the figure of von Neumann can be made. More in general, historical analysis could provide useful indications for the future of the model of economic and cultural development of advanced industrial societies. This is even more true since the problems of defense and international security do not seem to have cleared the field, even after the end of the Soviet system, and especially after September 11, 2001; since the idea that global economy and finance is under control has been radically challenged by the collapse of financial markets; and since the acceleration of technological and scientific development continues to pose unheard of challenges on the moral and political plane.

4.8 Von Neumann's final years: a very engaged expert and the time stealthily borrowed for the scientist's projects

Von Neumann's feverish activity in the postwar years did not stop short at collaboration with the Los Alamos Laboratory and RAND and his position as an expert on government committees. Many private companies in the USA endeavoured to get him to work for them, such as Ramo-Wooldridge, IBM and the Standard Oil Development Company. IBM offered him a contract for the first time in May 1945, and he was authorized to accept it both by the IAS director and by the government agencies. However, in the end, he decided to pass it up and concentrate on the project of constructing a calculator at Princeton. In 1947 he signed a contract with Standard Oil for consultancy on oil prospecting, production and refining: he was paid 6,000 dollars for a maximum of thirty days work per year. In the course of this collaboration, he carried out important studies in fluid dynamics applied to oil prospecting. In particular, his main ideas concerning the analysis of the stability of numerical solutions of partial derivative equations, which he failed to publish, are contained in one of his reports to Standard Oil in July 1948.[121] Later, in 1951, he began collaborating with IBM, on a contract that demanded the same time commitment as the preceding one but guaranteed him 12,000 dollars a year plus expenses. IBM was interested in obtaining his advice on a wide range of problems, from improvement of the efficiency of its plants using linear programming methods, to development of information tech-

120) Hughes 1998, 10. Several US historians have claimed that science and technology underwent radical conditioning exerted by the military context on the organization, guiding ideas and even research contents and lines (see for example Edwards 1996, Leslie 1993). There is a more or less explicit evaluation of a distortion and thus of a negative impact by the military-industrial-academic complex on science, which is the counterpart of the thesis (the "crowding out" of civil R&D) that it also had a negative impact on US industrial development. In Europe the postwar choice was to resolutely abandon research and development in the military field despite the spread of state intervention and regulation of economic activities. Therefore this debate has not been developed.

121) Neumann (von) 1948, unpublished report printed in JNCW, vol. V, 664–712. See the analysis of this paper in Aspray 1990, 105 ff.

nology systems having reliable low cost memory storage, and even planning of new products.

Even with his many commitments as public and private consultant, he found the time to develop new scientific projects, often linked to the new interests that arose during the wartime period. After ending his collaboration with the Moore School – and with the final controversy that this entailed, as previously described – he threw himself into another project, which has already been mentioned, the construction of the ECP calculator at IAS. On this occasion, he looked after the whole project: theoretical and engineering aspects, planning, the organization of the group of scientists and engineers, fund raising, and other administrative aspects. Convinced as he was of the enormous potential of the use of computers in scientific research and of the changes this would bring about, his main objective was to build a machine suitable for scientific activity. The ECP was used for design and application of numerical analysis methods developed by von Neumann and his group which were tested in applied problems such as those related to the numerical solution of partial differential equations in turbulence theory and meteorology.

This interest in applied mechanics, numerical analysis, optimization theory, mathematical statistics and other topics, several of which involved radically innovative mathematical techniques, was not of course peculiar to von Neumann alone. The various research programs developed during the war produced a spectacular boom of applied mathematics in the United States. In 1929 Veblen had opposed the creation of a specialized journal because he did not believe that applied mathematics existed as such. However, this situation changed rapidly and applied research – which has historically always been linked to the theoretical aspects of mathematical research – gradually took on a well defined profile of its own, which partly conflicted with the strongly abstract nature of postwar "pure mathematical research", particularly under the effect of the spread of the Bourbaki movement.

The AMS undertook to coordinate the interest in applied mathematics: although the proposal to create a specific section for applied mathematics did not eventuate, an ad hoc Committee – of which von Neumann was a member, together with Richard Courant, a renowned mathematical physicist who had previously been his professor at Göttingen and had founded after the war the Institute of Mathematical Sciences at New York University. In 1947 the Committee organized a workshop at Brown University on non-linear problems in continuum mechanics, the first of an AMS series of specific symposia on applied mathematics. Later, in 1952, the SIAM (Society for Industrial and Applied Mathematics) was founded.

The *Symposia in Applied Mathematics* series was the first example of the growing diversification of activities of the AMS, which appropriately represented the importance of the US mathematical community, which now dominated the world scene. In this sense, it was symbolic that the first International Congress of Mathematicians held after the interruption due to the war should be held in 1950 at Harvard University and be chaired by Veblen, while von Neumann was a member of the finance committee. At the same time, a committee formed by members of the AMS and other associations (the MAA, the Institute of Mathematical Statistics and the Association

for Symbolic Logic) set about refounding the International Mathematics Union. Only Soviet mathematics succeeded in rivalling US mathematics in importance and quality, although it followed very different lines of approach and methods. The internationalist ideal was now a fundamental component of scientific practice, although it was seriously hindered by the Cold War and scientific contacts among mathematicians in the two countries were very difficult. The institutional initiatives regulating these contacts – including the activity of translating Russian publications, funded by the ONR and organized by the AMS starting in late 1947 – did not prevent scientific relations from being awkward and contacts infrequent. This accounts for the scarce dissemination of Soviet research on probability theory and on mathematical physics – in particular, the development of qualitative analysis of ordinary differential equations by the school of Andrej N. Kolmogorov – or the delay in disseminating the formulation of linear programming in the work carried out by Leonid V. Kantorovič starting in the late 1930s.

Von Neumann played a role of fundamental importance in the boom in new applied mathematics research topics in the 1940s and 1950s: his forays into a wide range of fields were always a source of inspiration to advanced researchers. Many of these topics, born out of wartime necessity, were linked to engineering issues, but also to problems related to the social sciences and biology. Nevertheless the central theme of his interests in this period was the development of a theory of automata considered as a development of logic and to be closely linked to the theory of calculators, which could also provide heuristic models in genetics. In 1948, he presented to the Hixon Symposium on "Cerebral Mechanisms in Behavior", a work on the general theory and logic of automata. He later continued to lecture on these topics although most of his work was not published. Although his many public and institutional commitments prevented him from concentrating on his scientific activity, everything seems to suggest that he had in mind to organize a future for himself entirely dedicated to research. His lecture delivered to the International Congress of Mathematicians held in Amsterdam in September 1954, a few months before his appointment as a member of the AEC, is an indication of his tireless interest in the more obscure logico-philosophical aspects of mathematics: indeed he concerned himself with several aspects of the theory of operators linked to problems raised in quantum theory and in particular with the axiomatic formalization and relations between logic and quantum probability.[122] For the time being, his work as a member of the AEC demanded a fulltime commitment and entailed renouncing all his public and private consultancy commissions and activities, except for the von Neumann Committee on strategic missiles. He had now reached a peak in a phase of his life and everything seems to indicate that, as early as 1955, he was already planning his future well beyond his term of office at the AEC, which was of four years duration.[123]

122) Rédei 1999. The same year, as Rédei points out, Dieudonné proposed the name of "von Neumann algebras" for algebraic structures still called "ring of operators".

123) See Aspray's reconstruction (1990, 251) and von Neumann's letter to James R. Killian, president of the Massachusetts Institute of Technology, of February 24, 1956 (printed in Rédei (ed.) 2005, 165–166).

Would it have really been possible for a person so deeply involved in advisory and institutional activities to return to research in an academic environment? In any case, the IAS no longer represented for him an ideal work centre: it was an institute with a vocation for abstract scientific research which had barely tolerated, amid strong resistance, the development within it of a project for computing and numerical analysis: the ECP had now been completed and at the end of this phase the group formed by von Neumann was about to be disbanded. Before this, Wiener had already suggested he transfer to MIT, an environment that was better suited to von Neumann's new interests. However, this prospect clashed with the extremely attractive possibility of moving to the West coast, where he had many scientific contacts, and joining the University of California, in Los Angeles.

In early 1955, von Neumann received a very flattering invitation from the Yale University: he had been nominated to deliver the traditional "Silliman Lectures" for the Spring term of 1956. As usual, in this busy period, he was forced to prepare his lectures in his spare time and, in view of his recent appointment to the AEC, it was necessary to ask Yale to reduce the lecture cycle to the duration of one week. As soon as he had agreed upon this with Yale, von Neumann accepted the invitation and it is not surprising that he should decide to profit by the lecture cycle to illustrate his recent studies on automata theory. But his plans were cruelly and brutally dashed, as his wife Klára recalls in her foreword to the published lectures:

> *On March 15, 1955, Johnny was sworn in as a member of the Atomic Energy Commission, and early in May we moved our household to Washington. Three months later, in August, the pattern of our active and exciting life, centred around my husband's indefatigable and astounding mind, came to an abrupt stop; Johnny had developed severe pains in his left shoulder, and after surgery, bone cancer was diagnosed. The ensuing months were of alternating hope and despair; sometimes we were confident that the lesion in the shoulder was a single manifestation of the dread disease, not to recur for a long time, but then indefinable aches and pains that he suffered from at times dashed our hopes for the future. Throughout this period Johnny worked feverishly – during the day in his office or making the many trips required by the job; at night on scientific papers, things which he had postponed until he would be through with his term at the Commission. He now started to work systematically on the manuscript for the Silliman Lectures; most of what is written in the following pages was produced in those days of uncertainty and waiting.* (Neumann (von) 1958, viii–ix)

Although aware of being seriously ill, he dedicated himself to the utmost to his AEC work. Indeed during the Spring and Summer of 1955 he had acted as acting chairman to replace Strauss, who had many other institutional duties. One of von Neumann's main objectives was for the AEC to contribute to the development of information science and technology: realizing that the time in which computers could be constructed

by individual scientific groups was now over, his idea was to fund private enterprises that were willing to enter this sector by offering study and research grants. However, his efforts in this direction were stymied by the worsening of his illness. By the end of 1955 the situation had deteriorated considerably: he was found to have several spinal cord lesions and began to have difficulty walking; by January 1956 he was in a wheelchair. Nevertheless, the hope remained that the treatment followed could at least slow down the progress of the disease. In March 1956 he signed an agreement to join the University of California when his term of office of member of the AEC came to an end. His job at the University was to consist of advising and collaborating with all the departments in the various campuses and in particular the departments of earth sciences, economics, management science, and engineering; the University's part of the bargain was to provide him with the best possible conditions for developing his research, particularly in the field of applications to geophysics and meteorology.

But his cancer was advancing inexorably and in April he was admitted to the Walter Reed Army hospital. Von Neumann continued to go to meetings in an ambulance and kept in touch by telephone with the AEC offices, assisted by his collaborator Paul Fine. In those months took place the dramatic events in his home country, Hungary, which, with the Soviet invasion, violently shattered the hope of any liberalization of the political regime. In recognition of his services to the United States, von Neumann was personally awarded the AEC Fermi Prize by President Eisenhower. The award was created at the proposal of Strauss and he was the first to receive it. In the same year, the President awarded him the Medal of Freedom. These were the last honours to be awarded to an extraordinary man. John von Neumann died in Washington on 8 February 1957 and was buried in Princeton.

Chapter 5

Beyond Mathematics: von Neumann's Scientific Activity in the 1940s and 1950s

5.1 From rational economy to the axiomatization of economic behaviour

A typical feature of twentieth century culture was the development of the social sciences. Towards the end of the nineteenth century, the study of social life, including the study of economic relations, was considered as essentially part of the so-called "moral and political sciences". This label emphasized the distinctly historical nature of these disciplines, which distinguished them from the natural sciences: nature is governed by laws, unlike history. In the twentieth century another idea spread, which had already taken shape in the preceding century in several research trends, but had been strongly opposed, namely, that the study of associated life phenomena must be embodied in a human and social sciences system: thus, economics, political science, sociology, psychology, anthropology would take their place side by side with the natural sciences in view of a gradual unification of concepts and methods. This led to the birth of a new category of scholars, the "social scientists", who, towards the middle of the century, were now present in relatively large numbers side by side with scientists and men and women of letters.[124] They felt called upon to offer their scholarship in practical contexts such as the management of national economies, activities analysis, the conduct of social groups, and the support of individuals in organizations (industries, schools, military units). In other words, the aim was to develop "soft"

124) See the considerations added in the 1963 edition of Charles P. Snow's well-known essay *The two cultures* (1959), in which he enthusiastically greeted the new presence that allowed the area of influence of the scientific method to be extended (Snow 1963).

technologies in the sphere of human and social problems that were based on solid rational knowledge, like the "hard" technologies that largely exploited the knowledge acquired in the natural sciences.

In the preceding chapter the participation of mathematicians, physicists and engineers in the US war effort during World War II was mentioned. During the war, many experts in the social and human sciences, anthropologists, sociologists and linguists, were part of interdisciplinary research groups organized by the government and the military authorities: for example, technical studies addressing the design of aircraft or the launching of bombs were accompanied by analyses referring to the behaviour and reactions of a pilot in the presence of enemy aircraft. The war work acted as a strong stimulus to the development of these branches and to their ultimate autonomous inclusion in the academic syllabus (in which only economics had hitherto enjoyed a consolidated position). Indeed, the social scientists collaborated in the war effort on a par with specialists in the "true" or "exact" sciences: there seemed to be no further doubts about the right of the social and behavioural sciences to enter the exclusive paradise of the natural sciences.

As we have seen, this epistemological equalization between natural and social sciences was based on the adoption by the latter of a methodology similar to that of the former, and even of basic concepts borrowed from them that were definitely extraneous to the classical historical and philosophical approach. The new social scientists – and the economists in particular – now had as their objective the pursuit of scientific objectivity, of stating general laws about society or human behaviour, and of demonstrating their predictive capacity. And it was inevitable that these laws would have to take on a mathematical form. The interest of some mathematicians and physicists in these attempts played an important role, especially in the consolidation of the new image of the social sciences and the confidence of those following the new approach. Von Neumann's foray into this field had particularly significant repercussions.

It must be made clear that, although this process was speeded up by scientific developments related to the war, it also had deep and historically remote roots. Ever since the late eighteenth century, there had been a gradually emerging effort to extend to all fields of knowledge the scientific method that had led to such breakthroughs in astronomy, mechanics and mathematical physics. One of the fundamental trends was to "mathematize" a wide range of different disciplines, in imitation of the mechanics model in particular, the laws of which had been formulated in the language of differential equations. This project developed throughout the nineteenth century but ran into strong opposition stemming from the dominance of romantic conceptions calling for the need to maintain a clear-cut distinction between the sphere of phenomena of inanimate nature, dominated by objective laws that can be expressed in mathematical language, and the phenomena of life and man, in which subjectivity and freedom prevail. Attempts to mathematize biological, social and economic processes encountered widespread and often bitter opposition by the champions of moral and political sciences as well as among natural scientists themselves.[125]

125) See Israel 2004b and, for economics, Ingrao, Israel 1990.

Nevertheless, the idea of constructing a mathematical economics had a number of distinguished followers, including above all Léon Walras, who formulated a theory of economic equilibrium that was to become the cornerstone of a "rational and experimental" social science based on the model of mathematical physics. The very name of the theory suggests an analogy with the mechanical concept of equilibrium. Walras described an ideal market situation in which competition among economic agents leads to a state of compatibility of their different actions: this state consists of equilibrium between demand and supply. Walras' aim was to establish in quantitative mathematical terms the existence and the properties of economic equilibrium, including the uniqueness of equilibrium and its stability as defined in a dual acceptance: "local", in the sense that the system whose equilibrium has been perturbed tends to return to the state of equilibrium; and dynamic or "global", in the sense that the market has the virtue of returning the economic system to equilibrium starting from any initial state. To achieve this, he formulated a theory of "value" or of the price of goods as a function of the "marginal utility", that is, of the scarcity of the goods themselves. This theory was intended to ground political economy on a scientific basis having the same dignity and rigor as that of the physico-mathematical sciences: thus, owing to the nature of its objective, economics would have deserved the label of "psycho-mathematical" science.[126]

This project of an economics written in the language of the theory of functions and infinitesimal analysis aroused harsh criticism from the majority of economists, who were attached to the historical research method and rejected any claim to translate human "freedom" into numbers. For their part, mathematicians not only displayed a similar scepticism but also criticized the paucity of mathematical results obtained so far. Indeed decades of research had led to the formulation of a system of non-linear equations, the solution of which was supposed to determine the equilibrium: however, the only argument put forward up to then by Walras and his school to prove the existence of such a solution was the manifestly fallacious one that the number of equations equalled the number of unknowns. Neither had the research of Vilfredo Pareto, who succeeded Walras in his chair at Lausanne, although carried out with greater mathematical competence, led to any substantial analytical progress. In actual fact, by the end of the first decade of the twentieth century, the theory of economic equilibrium was in serious crisis. The paths to be followed in the analogy with mechanics were by no means clear. Furthermore, this was happening at a time in which the reductionist and mechanistic approach was in crisis also in physics. Pareto, who had pursued the Walrasian program radically accentuating the mechanistic approach, had ultimately to surrender in the face of insurmountable mathematical difficulties, as well as of the failure of the attempt to put the theory on a solid empirical footing, an attempt that had induced him to enter the field of sociology.[127]

126) See Walras 1909. Walras' program, stated in its entirety starting in the 1870s, was based on the preceding research of Antoine Augustin Cournot (Cournot 1838), the origins of which dated to the beginning of the century; see Ingrao, Israel 1990.

127) Pareto attempted to make an almost scholastic analogy between economics and mechanics: *homo œconomicus* vs. material point, "ophelimity" vs. potential, etc. Lastly, he very lucidly acknowl-

Despite these difficulties, the theory of general economic equilibrium gradually caught on in the economists' environment as a way of conceptualizing the competitive market. Some of them, without claiming to provide a detailed account of the mathematical aspects, had emphasized its merits as a theory that could be used to provide a unitary and global interpretation of the economic processes occurring in the market, and its potential as a heuristic model. This approach aroused both consent and constructive interest. The discussion of the comparative advantages and disadvantages, respectively, of a market economy and of a planned economy, stimulated by the establishment of a socialist regime in Russia, reawakened attention to the theory. It was above all in the Anglo-Saxon environment that it gained a much stronger footing than it had achieved in Continental Europe. On the other hand, the related mathematical problems represented a true challenge for the flourishing Central European mathematical school: the prospect thus gradually emerged of tackling these problems according to an axiomatic approach and abandoning the mechanistic approach that had marked the contribution from the Lausanne school.

It is not at all strange that the Vienna mathematicians were the first to take an interest in these topics. Vienna was the most open and interdisciplinary of the intellectual centres in the period between the two wars. It was precisely in the environment of the Vienna Circle that the ancient desire to unify science and scientific language had re-emerged and that an explicit formulation was given to a program for the refounding of psychology and the social sciences according to the principles of behaviourism or of methodological materialism. Indeed the construction of unified science would have to include «sociology as well as chemistry, biology as well as mechanics, psychology – more properly termed "behavioristics" – as well as optics»[128] in a unified language. The two attributes of the "scientific conception of the world" outlined in the Vienna Circle manifesto were an empiricist and positive knowledge based on immediate data and application of the logical analysis method.[129] Only logical empiricism, once the old metaphysical concepts and the "rough" transfer by analogy of the principal features of the natural sciences to the social sciences have been put aside, would allow the "backwardness" of the latter to be overcome. In the mid-1930s a concrete proposal was therefore made – to publish an *Encyclopaedia of unified science*. Otto Neurath explained the meaning of this project during the international conference on scientific philosophy held at Paris in 1935:

---

edged the difficulties involved. In his *Manual of political economy* (Pareto 1906) he explains that the hypothesis of quantifying the ophelimity and the circumstances of the production of goods is "absurd" – whereas in physics the claim to determine the quantities involved in the equations of motion is not – and leads to such large analytical difficulties that "the only way of resolving this system of equations would be to observe the practical solution provided by the market". In this way, "it would no longer be mathematics that can come to the aid of political economy but political economy that will come to the aid of mathematics".

128) Neurath 1932–33, quotation from the English translation, p. 207.

129) The manifesto *Wissenschaftliche Weltauffassung. Der Wiener Kreis* was published by the Verein Ernst Mach in 1929 (Hahn, Neurath, Carnap 1929). See also Carnap's and Neurath's papers on the journal *Erkentniss* in years 1930–33, available in English in Ayer (ed.) 1959.

> *The aim was not so much to split hairs over disciplines that were already quite mature, but rather to concern oneself with branches that have so far been somewhat overlooked, such as psychology, biology and sociology. One of the important tasks of this Encyclopedia is to show to what extent these disciplines can share with physics a single common language, and how the laws of the different sciences nevertheless display distinctive peculiarities.* (Neurath 1936, 59)

When describing the early stages of von Neumann's life, we mentioned his contacts with the Vienna environment and in particular with Karl Menger's mathematical *Kolloquium*. Menger closely followed the discussions of the Vienna Circle, although he was very critical of the mathematical conceptions and language that predominated in it. In particular, his position on intuitionism and on the issue of the foundations of mathematics was very close to that of Hilbert and his school, including von Neumann. Son of the well-known economist Carl Menger, he also maintained frequent close contacts with the economists' environment: at his instigation, the mathematical problems involved in the theory of economic equilibrium became an important issue in the *Kolloquium* in the 1930s.[130]

In 1931 Menger extended an invitation to participate in the discussion to Karl Schlesinger, a Hungarian banker and economist, who had settled in Vienna after the Béla Kun revolution. Schlesinger had followed a marginalist approach in his work on monetary theory and his reflections led to a new formulation of Walras' equations being considered. Menger put him in touch with the young mathematician Abraham Wald, who was born in the Romanian city of Cluj – which had been part of Hungary ever since the end of World War I – and who was just finishing his university studies at Vienna. Wald first gave Schlesinger some lessons in mathematics, collaborating with him on the problem of how to solve equilibrium equations. In the 1933–34 volume of the Kolloquium proceedings, the series *Ergebnisse eines Mathematischen Kolloquiums* (which Menger edited after emigrating to the United States under the title "Reports of a mathematical colloquium"), Schlesinger presented his system of equations and announced that Wald had demonstrated the existence and uniqueness of the solution of this system; in the following pages Wald set out the first proof of existence in the history of the theory of economic equilibrium. Subsequent discussion led Wald to perfect his results: his line of research was based on the use of the axiomatic method, that is, on the introduction of suitable conditions defined in mathematical terms by means of equations and inequalities, although it essentially made use of classical infinitesimal techniques.[131] Moreover, the conditions in which Wald demonstrated his results were rather restrictive and distant from full generality.

130) In this reconstruction of von Neumann's interest in economics and social science in the 1930s, we follow Ingrao, Israel 1990, 177 ff.; Weintraub 1983; Leonard 1995, 1998.

131) An unsuccessful attempt to demonstrate the theorem of the existence of equilibrium had been made by the Italian mathematician Gaetano Scorza in 1903 (Scorza 1903). His attempt shows that mathematics at that time still lacked a fundamental tool: the fixed point theorems, the principal of which – Brouwer's theorem – was proved only in 1910. Wald's works were all published between 1934 and 1936. See Ingrao, Israel 1990 (including a complete bibliography); a detailed reconstruction of

One of the most attentive readers of Wald's work was von Neumann. Mention has already been made of von Neumann's interest in economics, and of the cultural, social and also family roots of this interest, and we have also mentioned Kaldor's and other people's evidence according to which von Neumann was familiar with the main principles of Walras' theory and held a critical attitude towards it. In Chapter 2 we recalled how his 1928 article on game theory drew an analogy between the latter theory and Walras' microeconomics: it could actually be said that in that work *homo ludens* was equated with *homo oeconomicus*. However, to tell the truth, this is the only non-controversial phrase uttered by von Neumann concerning Walras' approach that we are aware of. In the rest of his work and in all the known evidence regarding his ideas on the matter, every mention of the theory of general economic equilibrium has a negative connotation.

For the purpose of reconstructing the development of his reflection on economic theory, evidence may be drawn from Harold Kuhn and Albert Tucker, according to which, in 1932, he presented to the IAS of Princeton the economic model of linear growth already referred to in Chapter 2 (Kuhn, Tucker 1958). This result was never published and was left lying in a drawer. Just over one year later, it was the knowledge of Wald's results that drew von Neumann's attention again to this model. As Menger wrote:

> *Wald's paper on the equations concerning production greatly interested von Neumann, as he told me when passing through Vienna soon after its publication. It reminded him of equations he had formulated and solved in 1932 and now offered to present to our Colloquium.* (Menger 1973, 55)

Von Neumann's work, entitled "On a system of economic equations and a generalization of Brouwer's fixed point theorem" was published in 1937 in the *Kolloquium proceedings*.[132] In this paper the axiomatic approach previously adopted by Wald was taken to its extreme consequences, leaving aside any concern over the economic interpretation of the model. He eliminated all distinction between factors and products, and reduced an economy to a formal mechanism for the transformation of goods by means of given processes; he then introduced a series of conditions expressed by means of linear equations and inequalities whereby he was able to demonstrate the existence of a price equilibrium vector. The technique used in the demonstration was extremely original, as the problem was reduced to a minimax problem and the solution obtained by means of a generalization of Brouwer's fixed point theorem via a mathematical procedure similar to that used to demonstrate the results of his 1928 article on strategic games.

Further evidence regarding von Neumann's ideas on the mathematization of economics is offered by a letter sent by him to Flexner, the director of the IAS, from Budapest, in May 1934. Flexner had sent him Georges and Edouard Guillaume's

the events leading up to the demonstration of the fundamental theorem for the Walrasian theory (the theorem of the existence of equilibrium) may be found in Weintraub 1983.

132) Neumann (von) 1937; republished in English as Neumann (von) 1945b.

book *L'Economique rationnelle* (1932).[133] Von Neumann replied with a detailed and pitiless criticism of the mathematical treatment given by the French authors. This criticism is worth mentioning as it shows how his point of view was still intermediate with respect to the one that, as will be seen, he was later to adopt:

> *I think that the basic intention of the authors, to analyze the economic world, by constructing an analogical fictitious model, which is sufficiently simplified, so as to allow an absolutely mathematical treatment, is – although not new – sound, and in the spirit of exact sciences. I do not think however, that the authors have a sufficient amount of mathematical routine and technique, to carry this program out. I have the impression that the subject is not yet ripe* (*I mean that it is not yet fully enough understood, which of its features are the essential ones*) *to be reduced to a small number of fundamental postulates – like geometry or mechanics. The analogies with thermodynamics are probably misleading. The authors think, that the "amortization" is the analogon to "entropy". It seems to me, that if this analogy can be worked out at all, the analogon of "entropy" must be sought in the direction of "liquidity". To be more specific: if the "analogon" of energy is the "value" of the estate of an economical subject, then analogon of its thermodynamical "free energy" should be its "cash available". The technique of the authors to set up and deal with equations is rather primitive, the way f. i. in which they discuss the fundamental equations* [...] *is incomplete, as they omit to prove that 1: the resulting prices are all positive* (*or zero*)*; that there is only one such solution. A correct treatment of this particular question, however, exists in the literature.*[134]

Here there is no outright rejection of the attempt to construct concepts similar to those of physics, although its primitive character is criticized above all from the mathematical standpoint. Von Neumann was later to claim that a worthwhile foundation of mathematical economics should be based on the rejection of mechanical and physical reductionism and on the creation of a new kind of mathematics better suited to the specific nature of socio-economic phenomena. The evolution of his views on this issue took place in the years that followed. But in the meantime, the Kolloquium activity was brutally interrupted by the events in Europe: Menger migrated to the United States in 1937; Schlesinger committed suicide on the day of the entry of nazi troops in Vienna, in March 1938; Wald, after many vicissitudes, managed to flee to the United States, where he no longer concerned himself with these topics and dedicated himself to the application of statistics to economics and management.

133) Guillaume, Guillaume 1932. Several years later the same authors published a revised version of the work: Guillaume, Guillaume 1937.

134) Neumann (von) to Flexner, May 25, 1934, IAS Archives, Faculty Files, John von Neumann, Folder 1933–35.

5.2 The theory of games: a new mathematics for the social sciences

As fate would have it, the spirit of the Vienna environment was reborn at Princeton thanks to the meeting of von Neumann and Oskar Morgenstern, who had also participated in Menger's Kolloquium and was close to the conceptions of logical empiricism. Morgenstern, a distinguished economist of the Austrian school, editor of the journal *Zeitschrift für Nationalökonomie* and director of the Institut für Konjunkturforschung, had been removed from all his posts as a politically suspect person: in 1938, he left Vienna and joined Princeton University. He held extremely critical opinions on the state of economic theory: at Vienna he had gradually distanced himself from the economists' environment and developed a keen interest in Menger's ideas. At Princeton he preferred the company of the great physicists and mathematicians to that of his colleagues in the department of economics. Very soon his main interlocutor became von Neumann: the upshot was the beginning of a series of intense discussions on their favourite philosophical and scientific topics, and a strong friendship developed. Von Neumann illustrated to Morgenstern many aspects of his preceding studies such as quantum mechanics, the debate on the crisis of the foundations of mathematics, and Hilbertian axiomatics. He recommended to him various textbooks on mathematics and Morgenstern applied himself to these studies, although with some difficulty.

Gradually their discussions began to focus on topics of economics and game theory. Morgenstern explained to von Neumann his criticism of the theory of general economic equilibrium and, in particular, of one of its basic tenets, that of an economic agent capable of perfect prediction. In the last works he published in Vienna, he had insisted on the need to adopt an axiomatic approach to arrive at a rigorous and exact treatment of economic phenomena: the essential condition for economic theory to progress was the renunciation of classical analogies with physics and the use of logic, the common basis of all sciences. Morgenstern and von Neumann were in perfect agreement on this point of view. Moreover, Morgenstern essentially viewed the new techniques introduced by von Neumann in his analysis of games and of production as a new mathematics that was particularly well suited to the social sciences.

The conceptual system Morgenstern had in mind for the purpose of implementing his projects was the one proposed by Menger in his book *Moral, Wille und Weltgestaltung. Grundlegung zur Logik der Sitten*, published in 1934.[135] Menger had become involved in ethics after reading Wittgenstein and followed a radically different approach from the conventional one of moral philosophy. His aim was not to respond to the ultimate questions concerning the concept of ethics or of good since – he claimed – no universal ethical systems exist that can regulate relations among individuals. It was rather a question of making a logical analysis of the consequences of adopting various systems of ethical rules and of the decisions to which they lead. Menger analyzed a number of examples of association or division within a group of

135) Menger 1934; Morgenstern 1936.

human beings caused by the adoption of different regulatory systems – using criteria that could be called ethical, but also aesthetic, political or economic – by means of logical-combinatorial considerations that had nothing to do with the so-called "higher mathematics", that is, with classical infinitesimal calculus. The approach of the optimization of strategies introduced by von Neumann via the use of minimax techniques, both in his analysis of market equilibrium and in the study of game strategy, suggested a kind of social analysis very similar to Menger's. Morgenstern considered that the analysis of the subjects' behaviour under the influence of the effects of social interdependence would remedy the flaws in economic theory.

Von Neumann and Morgenstern began to prepare a work in collaboration on these bases. Von Neumann had developed his studies on the analysis of game strategy; he continued to give lectures on the subject but had not published new research. The initial idea behind the collaboration was for Morgenstern to write an introduction to the economic interpretation of von Neumann's studies on the theory of games. In Autumn 1941 there was so much accumulated material that the two decided to publish a book, the final version of which was ready in January 1943. The book, *Theory of games and economic behavior*, published in 1944 (2^{nd} ed. 1947, 3^{rd} ed. 1953), started out from a radical criticism of the Walrasian theory of economic equilibrium, especially for gaps in the description of the influence exerted on the decisions and behaviour of each individual by reciprocal interactions with other individuals; these interactions and influences should instead be considered as guiding the individual's actions in his attempts to obtain maximum advantage. Von Neumann, in particular, always vigorously insisted on the idea that the type of optimization stemming from the game theory approach was much more complex and rich than the optimization of classical mathematical analysis: the latter took in account only general parameters, while, in game theory, the agent makes his choices on the basis of behaviour of the other agents.

The intense work of the two distinguished immigrants had led to a treatment of an impressive scope. From the outset, it proposed a new theory of utility, reformulated all the basic results of game theory, with special reference to the minimax theorem for mixed strategies, and contained numerous applications, including a bluff theory for poker. The interpretation of the mixed theories in regulatory terms (compared with the other frequentist and psychological approaches that we have described in Chapter 2) was again proposed very clearly:

> [...] *one important consideration for a player in such a game is to protect himself against having his intentions found out by his opponent. Playing several different strategies at random* [...] *is a very effective way to achieve a degree of such a protection. By this device the opponent cannot possibly find out what the player's strategy is going to be, since the player does not know it himself.* [...] *Ignorance is obviously a very good safeguard against disclosing information directly or indirectly.* (Neumann (von), Morgenstern 1944, 146)

The book did not deal with the problem of a game with *n* players and thus there was no generalization to this case of the minimax theorem, until Nash did so several years later. The cases with more than two players were mostly reduced to the two players case by taking into consideration the coalition agreements that reduced the number of adversaries. It is important to note that this choice did not represent an *ad hoc* stratagem but corresponded to a general view reached by both authors, namely that the cooperative approach – in which the players joined forces in groups in order to achieve common goals – was much more significant than the non- cooperative approach in which each player acts quite independently, that is, in a way that quite closely resembles the view inherent in the theory of general economic equilibrium.

The reasons underlying this preference for the cooperative approach were fully clarified by Morgenstern in an article he wrote some thirty years later:

> *Interdependence has, of course, been recognized, but even where neo-classical economics of the Walras-Pareto type tried to describe this interdependence, the attempt failed because there was no rigorous method to account for interaction which is evident especially when the number of agents is small, as in oligopoly* (*few sellers*)*. Instead large numbers of participants were introduced* (*under the misnomer of "free competition"*) *such that asymptotically none had any perceptible influence on any other participant and consequently not on the outcome, each merely facing fixed conditions. Thus the individual's alleged task was only to maximize his profit or utility rather than to account for the activities of the "others". Instead of solving the empirically given economic problem, it was disputed away; but reality does not disappear. In international politics there are clearly never more than a few states, in parliaments a few parties, in military operations a few armies, divisions, ships, etc. So effective decision units tend to remain small.* (Morgenstern 1973, 266–67)

There is considerable evidence that von Neumann developed an increasingly sceptical attitude to the non-cooperative approach, above all after the demonstration of Nash's theorem in 1951 solving the general case of *n* players. In evidence provided by Nash himself to Sylvia Nasar, von Neumann greeted Nash's result very coldly, deeming it a simple fixed point mathematical theorem. There is further evidence from Martin J. Shubik, mentioned by Philip Mirowski, to the effect that, on a train journey in 1952 from New York to Princeton, Shubik met von Neumann and told him that he considered Nash's non-cooperative equilibrium solution with great favour (and as being suitable for economic applications): von Neumann answered that he did not like Nash's solution and felt that a cooperative theory made more social sense. Moreover, Nash himself, in an interview with Robert Leonard, admitted that a cultural difference existed between himself and von Neumann and Morgenstern, in that the latter were probably inspired by a more "European" type of social model, while he was influenced by an outlook typical of "American" individualism.[136]

136) Nasar 1998; Mirowski 1992; Leonard 1995.

In any case there is no doubt that, only a few years after the publication of *Theory of games and economic behaviour*, Nash's works in the 1950s established the premises for a split in the directions taken by game theory. Von Neumann became increasingly favourable to the cooperative approach. Indeed his thinking on this subject even took on holistic overtones which revealed his growing sensitivity to the topic of complexity. In a letter he wrote to Kuhn in April 1953 he even went so far as to say: «I think that nothing smaller than a complete social system will give a reasonable "empirical" picture».[137] Many years after von Neumann's death, in the already cited article, Morgenstern illustrated this point of view as follows:

> *Clearly a comprehensive theory of decision-making would encompass virtually all of the voluntary human activity and as such would be an absurd undertaking, given the infinity of human situations. A more reasonable approach is to develop a science, or sciences, dealing with the principles, so as to govern decision-making in well-defined settings. In what follows the structure of that theory will be laid bare as far as this is possible without going into the use of the underlying mathematics. Game theory represents a rigorous, mathematical approach towards providing concepts and methods for making reasonable decisions in a great variety of human situations.* [...] *More generally* [...] *no complete formalization of society is possible: if a formalization is made, it is either incomplete or self-contradictory.* [...] *Every social theory must therefore be dynamic, proceeding from one formalism to another.* (Morgenstern 1973, 274)

However, in the meantime, research in the field of game theory had mainly adopted the non-cooperative approach indicated by Nash's results. This did not happen immediately, although by the end of the 1970s it had become a consolidated approach. In one sense, this development implied a rapprochement between game theory and the theory of general economic equilibrium, which – with the work of Kenneth J. Arrow and Gérard Debreu (Arrow, Debreu 1954) – made use of the results of von Neumann and Nash in a strictly technical sense (as a mathematical tool) and then discarded their formulation in terms of game theory (Debreu 1959).

The assessment of the fruitfulness of this approach is an open question.[138] What is clear, from the historical point of view, is that the path followed by the theory was not the one recommended by von Neumann. The characterization given by Robert J. Aumann on Nash's equilibrium as a perfect representation of the idea of economic equilibrium and the rationality of economic agents[139] was precisely the one that von

137) Neumann (von) to Kuhn, April 14, 1953, Von Neumann Papers, Library of Congress, published in Rédei (ed.) 2005, 170.

138) Mirowski makes a very severe assessment of Nash's approach, saying that it completely destroys the interactive idea underlying von Neumann's game theory: it «demands the complete and total reconstruction of the thought processes of the Other – *without* communication, *without* interaction, *without* cooperation – so that one could internally reproduce (or *simulate*) the very intentionality of the opponent as a precondition for choosing the best response». (Mirowski 2002, 343–44).

Neumann would have advanced in justifying the unacceptability of this concept. Indeed, for von Neumann, the originality of game theory consisted precisely in the clear-cut progress it offered compared with the classical procedures of optimization. In a recent article, the economist Paul Samuelson judged von Neumann's and Morgenstern's work:

> *John von Neumann and Oskar Morgenstern had complementary properties that led to a fruitful cooperative product.* [...] *But, it can be said in a low-keyed way, there was something in their respective scientific personalities that led to a resonance of minor faults – some tendency towards nihilism combined with Napoleonic claims. Schumpeter was pleasantly excited by von Neumann's claim that economics would need new mathematics different from the mathematics developed for physics after Newton's time* [...] *However, except for the philosophical complications introduced by games involving more than one person, I do not honestly perceive any basic newness in this so-called non-physics mathematics. When Clerk Maxwell and I – let the clause stand – describe melting points of ice in thermodynamics, we benefit from the Debreu-von Neumann novelties and still operate in Isaac Newton's style.* (Samuelson 1989, 211)

Von Neumann had actually responded in advance to this judgment in a sharp comment contained in a letter written to Morgenstern in 1947, in which he refused to review Samuelson's *Foundations of economic analysis* on the grounds that it was based on a primitive mathematics:

> *You know, Oskar, if those books are unearthed sometime a few hundred years hence, people will not believe they were written in our time. Rather they will think that they are about contemporary with Newton, so primitive is their mathematics. Economics is simply still a million miles away from the state in which an advanced science is such as physics. Samuelson has murky ideas about stability. He is no mathematician and one should not ascribe the analysis to him. And even in 30 years he won't absorb game theory.*[140]

This conflict clearly explains the paradox of the reception of *Theory of games and economic behaviour*. On the one hand, it raised very great interest and had an enormous influence on the paths taken by the mathematization of economics and of the social sciences. But as a matter of fact many economists – rather than displaying any true concern with the program proposed by von Neumann and Morgenstern – understood that, following this path, it was finally possible to successfully tackle some of

139) «The Nash equilibrium is the embodiment of the idea that the economic agents are rational: that they simultaneously act to maximize their utility; Nash equilibrium embodies the most important and fundamental idea in economics» (Aumann 1985, 43–44).

140) Neumann (von) to Morgenstern, October 8, 1947, quoted in Morgenstern 1976, 810.

their old and traditional problems, including those of the existence of general economic equilibrium. As we have said, this was achieved by Arrow and Debreu in the 1950s, but in particular on the basis of Nash's results. In this way, the intention to refound economics *ab ovo* on the basis of the theory of games was voided and the Walrasian type microeconomic approach regained the upper hand. The comparatively secondary role taken on by the cooperative approach was evidence of this research trend. This explains why the theory of games was developed above all in the context of mathematical economics research and with more of a supporting than a foundational role, while it failed to make progress in the world of mathematics research. One group of mathematicians worked actively at Princeton on developing the theory of games under the leadership of Albert W. Tucker and following the program laid down by von Neumann.[141] However, also in this field, von Neumann's involvement was drastically curtailed after publication of his fundamental book. For von Neumann, game theory, in so far as it was a mathematical theory, had to play a similar role for the economic and social sciences as that played by infinitesimal analysis in physics. Matters do not seem to have moved in this direction.

5.3 Decisions, organization, operations research

The cultural influence of von Neumann's theory of games extended far beyond the context of the theory of economic equilibrium, and even beyond that of economic thought. We have seen that the starting point of the book written with Morgenstern was actually a severe criticism of the approach followed by the Walrasian economic theory. However, what von Neumann was interested in was not so much the concept of equilibrium as one of the conceptual nodes of game theory, that is, the process of decision making, and more generally, the analysis of rational behaviour in social processes or social phenomena. Thus, the theory of games represented an effective concrete proposal of the idea backed by the Vienna Circle, that the only objective worldview is that obtained using the scientific method. Logical-mathematic analysis and experimental studies were therefore considered the only acceptable basis for the construction of modern human sciences. Von Neumann's and Morgenstern's treatment could well have been included in the Encyclopaedia of Unified Science which had remained incomplete after publication of several issues dedicated to the general idea of science outlined by logical empiricism. And it actually played a fundamental role in dissemination of these ideas during the second half of the century.

As Alain Finkielkraut (2005) has emphasized, these positions represented the outcome of an intellectual process asserting the mathematical and scientific method as the essence of knowledge and rationality itself, which represents one of the factors of modernity *par excellence* and became firmly established in the twentieth century.

141) The young researchers in the group included Harold Kuhn and John Nash. A number of works were published in the journal "Annals of Mathematics", and others collected in two volumes of the Princeton series *Annals of Mathematical Studies*: Kuhn, Tucker (eds.) 1950–1953. See also Luce, Raiffa 1957.

Moreover, a conception of this kind was not uncoupled from the development of other aspects of modernity, in particular from the development of industrial automation and technical networks; indeed the connection between the scientific conception of the world and the technical organization of the processes of modern production had been explicitly stated by Carnap, Hahn and Neurath in the Wiener Kreis 1929 manifesto.

As though echoing these auspices, game theory in the central decades of the century became the conceptual reference framework for a series of theoretical developments in sectors that were traditionally located outside the range of influence of mathematical methods: from military art to industrial organization, from politics to economic planning, to administration. Moreover, the philosophical positions of logical empiricism were compatible with a renewed confidence in the role of scientists and of the scientific worldview that led an innovative group of British and American scientists to the formulation and construction of *operations research*.[142] Under this label, which made a vague reference to operations, that is, to action organized in a collective environment, there were clearly basic models of operations in military art and of large-scale technical activities; however, the final goal was much more ambitious, something like a true applied social science or a social technology, all of which remained to be constructed. In Chapter 4 we have considered how game theory was explored by RAND's researchers. The practical outcomes were disappointing as regards both the study of aerial combat and of planning problems. However, other paths, often mathematically linked to game theory, were explored more successfully, such as linear programming, network flow analysis, and inventory control. All these mathematical techniques were developed for military purposes and later transferred to the solution of similar industrial and civil engineering problems. Von Neumann's theory of games, supported by his personality and authority, played a decisive role in the emergence of these novelties, which were destined to enjoy greater success.

The theory of games offered a reference framework by providing an abstract mathematical treatment – in which *axiomatic language* played an essential role – which could be applied in a wide range of sectors, depending on whether the players were interpreted as consumers, voters, negotiators, industrial suppliers or rival countries. It revived a classical idea of applied mathematics, namely *optimization* or the idea of formulating objectives typical of technical design in terms of maximization or minimization of a function. And yet, it radically modified this approach by replacing the tools of classical mathematical analysis with new ones such as inequalities, fixed point theorems and the theory of probability. The mathematical tools selected were explicitly indicated in the very first lines of the book *Theory of games and economic behaviour*: «The character of the procedures will be mostly that of mathematical logics, set theory and functional analysis» (Neumann (von), Morgenstern 1944, 3*rd* ed. 1953, ix). The theory of games provided also a new standard language that comprised

142) American scientists in the field of operations research were perhaps not really aware of the philosophical bases of such a scientistic position; in the United Kindom typical of many of the artificers of operational research (such as John D. Bernal, Patrick Blackett, and Robert Watson-Watt) was a kind of nineteen century scientism, that of Marxist philosophy (Hughes 1998, 146–149). This issue deserves further study.

concepts not derived from mechanics, such as those of decision-making, information, strategy and so on; and, lastly, it pointed to the dual individual-collective path in the analysis of human and social phenomena.

This influence is quite apparent in several developments dating back to the immediate postwar period, which were characterized by a massive deployment of mathematical tools that extended the range of mathematization far beyond the theory of economic equilibrium. These new mathematical operational tools provided an actual mathematical content to the label "operations research". The work carried out at the RAND Corporation, also in collaboration with the Cowles Commission for Economics Research at the University of Chicago with Air Force funding, has already been considered in Chapter 4. Other groups worked on contracts with the ONR, for instance at Princeton (the groups of Tucker and Morgenstern himself) and at Pittsburg at the School of Industrial Administration established in 1949 at the Carnegie Institute of Technology. Some of the more significant contributions were: Abraham Wald's theory of statistical decision; George B. Dantzig's formulation of the problems of planning expressed in terms of linear programming; Tjalling Koopmans' research on resources allocation problems; Kenneth J. Arrow's social choice research; the research on organization theory by Merrill Flood and Morgenstern himself; and the inventory control studies by the groups of Morgenstern, of the Carnegie Institute (including Herbert A. Simon) and many others.[143] All these authors had direct scientific relations with von Neumann and their work was carried out in institutions where his influence was felt. Several of the latter had already worked on operational problems (for instance, Koopmans' research on transportation or Simon's work on administrative behaviour); and yet, at the end of the 1940s, they opted decisively for the path of mathematization. This new approach to problems regarding the organizational sphere is effectively illustrated in a note contained in the Proceedings of the Conference on Mathematical Programming organized by Koopmans at Chicago in June 1949 (published two years later under the title of *Activity analysis of production and allocation*):

> *The cultural lag of economic thought in the application of mathematical methods is strikingly illustrated by the fact that linear graphs are making their entrance into transportation theory just about a century after they were first studied in relation to electrical networks, although organized transportation systems are much older than the study of electricity.* (Koopmans et al. (eds.) 1951, 258)

The evolution of twentieth century organization science and technology reveals a cultural turning point which brought back into currency the idea of rationalizing on a mathematical basis the decision-making and organizational processes, in the economic, industrial, military, political and administrative fields (Millán Gasca 2003, 2006). Von Neumann's influence in it must be assessed more thoroughly, which

143) Wald 1945, 1950; Dantzig 1951a, b; Koopmans et al. (eds.) 1951; Arrow 1951. On the work of Morgenstern and Flood on the theory of organization in the early 1950s at the RAND, see Leonard 2004; on research on inventory control funded by the ONR see Klein 1999.

demands, in addition to an analysis of the dissemination of the book written together with Morgenstern, the gathering of evidence regarding personal intellectual exchanges more than actual quotations or published work. One noteworthy and frank testimony was offered by Dantzig, which we quote below at length. It is particularly relevant as it refers to linear programming, what we could describe as a "soft technology" expressed in algebraic language. Linear programming underwent many extensions and was found to have direct mathematical structural links with game theory. Dantzig's account of his discussion with von Neumann is all the more convincing in that it transmits the idea of how capable the latter was of assimilating the specific concerns of each interlocutor and of providing them with mathematical "substance": the same as we saw in his collaboration with Morgenstern, the bearer of exquisitely economic interests.

In the case of Dantzig, von Neumann was up against a researcher with a solid mathematics background, the bearer of operational interests that emerged during the war, the harbingers of a great new category of large-scale management problems, themselves the result of an acceleration of technological development and mass production. Dantzig had gained considerable experience in solving logistic problems in the Air Force during the war. After a short period devoted to completion of his studies at Berkeley, he had begun working at the Pentagon on the "mechanization of the planning process", which consisted in finding a way of accelerating the calculation of a "scheduled programme of deployment, training and logistical supply" (Dantzig 1991, 20). The new opportunities offered by electronic computers made this work even more attractive, because it pointed to the possibility of effectively solving extremely complex problems on the basis of a precise mathematical formulation. He considered the problem of planning interdependent activities based on objectives stated in terms of *ends* rather than *means*, so as to eliminate subjective evaluations in organization decisions. Furthermore, he established the link between this problem and economic planning problems, as they had been formulated by the economist Vassily Leontief in his celebrated interindustrial input-output model of the US economy (dating to 1932). Towards the middle of 1947, Dantzig formulated the problem he had identified in mathematical theoretical terms, building an axiomatic framework for the abstract concepts of "activity" and "good". He thus found himself having to deal with an abstract mathematical problem, namely the maximization of a linear form (the objective function) subject to conditions expressed by means of linear equations and inequalities:

> *I proposed the simplex method in the summer 1947. But it took nearly a year before my colleagues and I in the Pentagon realized just how powerful the method really was. In the meantime, I decided to consult with the "great" Johnny von Neumann to see what he could suggest in the way of solution techniques. On October 3, 1947, I met him for the first time at the Institute for Advanced Study at Princeton.*
>
> *John von Neumann made a strong impression on everyone. People came to him for help with their problems because of his great insight. In the initial stages of the development of a new field like linear programming, atomic*

> *physics, computers, or whatever, his advice proved invaluable. After these fields were developed in greater depth, however, it became more difficult for him to make the same spectacular contributions. I guess everyone has a finite capacity, and Johnny was no exception.*
>
> *I remember trying to describe to von Neumann (as I would to an ordinary mortal) the Air Force problem. I began with the formulation of the linear programming model in terms of activities and items, etc. He did something which I believe was uncharacteristic of him. "Get to the point," he snapped at me impatiently. Having at times a somewhat low kindling point, I said to myself, "OK, if he wants a* quickie*, then that's what he'll get." In under one minute I slapped on the blackboard a geometric and algebraic version of the problem. Von Neumann stood up and said: "Oh that!" Then, for the next hour and a half, he proceeded to give me a lecture on the mathematical theory of linear programs.*
>
> *At one point, seeing me sitting there with my eyes popping and my mouth open (after I had searched the literature and found nothing), von Neumann said:*
>
> *"I don't want you to think I am pulling all of this out of my sleeve on the spur of the moment like a magician. I have just recently completed a book with Oscar Morgenstern on the Theory of Games. What I am doing is conjecturing that the two problems are equivalent. The theory that I am outlining for your problem is an analogue to the one we have developed for games."*
>
> *Thus I learned about* Farkas' Lemma *and about* Duality *for the first time.* (Dantzig 1991, 24)

A theoretical work produced by the interaction with Dantzig, "Discussion of a maximum problem", written in 1947, remained unpublished and was later edited by Kuhn and Tucker and published in his *Collected works* (Neumann (von) 1947b). Indeed, «a new theorem or a new calculation method could often lie for years in his archives, or be communicated solely during conversations with colleagues», according to evidence from Kuhn and Tucker (1958, 118). They recount that, in order to get him to publish one of his works – which opened up a new area of application of linear programming and game theory to combinatorial problems – decided to use a stratagem: von Neumann was invited to hold a seminar, in October 1951, and one of the participants wrote the work for him from notes that, with the title of "A certain zero-sum two-person games equivalent to the optimal assignment problem", was published in 1953 in a collection of papers included in the Princeton series *Annals of Mathematical Studies* (Neumann (von) 1953b). Again in 1954 von Neumann published an article in this field, "A numerical method to determine optimum strategy", in the *Naval Research Logistics Quarterly* which had just been founded by Morgenstern (Neumann (von) 1954b).

Von Neumann's interest in organization and decision issues kept pace with his attention to engineering problems ranging from energy exploitation to missile and computers development. Indeed, it has been seen how his involvement in the ICBM project brought him into contact with a specific decision-making scenario: the AF Strategic Missiles Evaluation Committee, which he headed, vigorously defended the importance of a "rational", scientific approach to the solution of organizational problems. However, wartime needs and applications requirements had caused his attention to be turned mainly towards numerical analysis and computing. In one sense, this subject represented a return to the origins, to the early problems of mathematical logic and of the foundations of mathematics, which he had abandoned after the wave of emotion produced by Gödel's results.

5.4 Engineering and mathematics: the project of an electronic calculator

World War II and the Cold War formed the backdrop to an unprecedented development of the radically new technologies that characterized the twentieth century, such as transport and communications technologies, automated systems control and automated information processing. Von Neumann's involvement with calculating machines was rooted in the opportunities they offered for development of numerical mathematical methods to solve applied real-world problems such as those described by hydrodynamics or linear programming models. But he was also well aware of the theoretical, philosophical issue raised by the development of computers: these machines seemed to realize the ancient dream of so many philosophers, engineers and poets that one day it would be possible to create artificial beings and reveal the mystery of the human mind. Digital computers catalyzed a return to this theme that was stimulated also by recent developments in mathematical logic and by progress made in neurophysiology and psychiatry. Several contributions in the 1940s-1950s opened the way to one of the most significant threads in the philosophical debate of the second half of the century and to the development of the so-called cognitive sciences – and here again von Neumann' s work was seminal.

In order to fully understand von Neumann's contribution to the development of the computer and information science, an often overlooked aspect must be borne in mind. His "initiation" to American electrical and electronic engineering – dominated in the early 1940s by the figure of Vannevar Bush and by the Bell Laboratories team – brought him into contact with a radically transformed technical thinking that had partly left the traditional empirical and concrete terrain and had set off decisively along the path of abstract mathematical formulation of problems.[144] Electricity supply, radio broadcasts, the telegraph, telephone, television: in the early twentieth century, in all these engineering contexts, in addition to the design of material technical devices (transmitters, receivers, channels), there were "intangible" problems that

144) See Millán Gasca 2005, 169 ff.

were formulated in mathematical terms, such as signal or information processing – described by means of transfer relationships between input and output – or network management (queues and so on).

In 1925, an engineer working for the American Telegraph and Telephone Company, John R. Carson, in a series of a lectures at the Moore School of Engineering, decidedly moved in this direction; the lectures were published one year later under the title *Electric circuit theory and operational calculus*. In 1929 Bush's *Operational circuit analysis* was published. In the years that followed the ideas put forward in these works led to an in-depth analysis being made of the mechanisms of regulation, stabilization and processing of the electric signal by means of an explicit exploitation of feedback processes in input-output models: as Stuart Bennett points out, these authors were considering «mathematical descriptions of the input-output relationships which ignore the actual physical process connecting the input and output signals» (Bennett 2004, 104). The most mature products of the work of the Bell Laboratories were Hendrik W. Bode's *Network analysis and feedback amplifier design* (1945) and the well-known *The mathematical theory of communications* (1949), written by Claude Shannon together with Warren Weaver.

The use of mathematics in engineering science was of course widespread in early twentieth century technology. Engineers used geometry to represent space and many basic concepts derived from physics were expressed in mathematical language. However, in the brand new communications engineering, the mathematization of technological thought advanced by a quantum leap, because mathematics was given a direct conceptual role – with no mediation by physics – in the theoretical formulation of problems. The basic ideas, such as system, network, feedback, control, transfer function, and information were strictly technological concepts defined in abstract mathematical terms. This use of mathematics to understand technical devices, operations, and processes weakened the intellectual role of physics in engineering and inevitably led to a move away from the classical approach in which mechanics had played a central role. One striking example is the case of one of the greatest radio engineers of the time, the Dutchman Balthazar van der Pol, whose work is an early manifestation of the emergence of the idea of *mathematical model*, as we have already seen in Chapter 3. These circumstances help us understand von Neumann's interest in electronics and communication engineering in connection with his view of the role of mathematics in knowledge that was no longer mediated by physics. For him, indeed, the role of mathematics in the understanding of physical phenomena, if viewed in a historical perspective, was a first complete example of mathematization, and that of other areas of phenomena would gradually follow. On the other hand, his work in the field of social sciences and engineering helps to understand the paradox we have dealt with in Chapter 3: his acceptation of the modelling approach, in a mathematician and scientist essentially faithful to a classical deterministic worldview.

It was Norbert Wiener, one of the most brilliant twentieth century scientists, who brought the new conceptions of communications engineering to the centre of a wide-ranging philosophical debate, which had profound repercussions in scientific and engineering environments in the central decades of the century and involved re-

searchers in a wide range of different sectors, including the biomedical and social sciences (Heims 1980, 1991). A mathematician by training, Wiener was a professor at MIT, one of the most advanced academic centres for technological research in the United States. Here he collaborated with Bush, thus gaining a knowledge of electrical engineering, and became interested in the development of computers. During the war, together with another engineer, Julian H. Bigelow, Wiener collaborated with the armed forces on the problem of anti-aircraft fire control, both in aerial combat and for defense against aerial bombing. The information available on the position and motion of an enemy aircraft (obtained by using another new technology – radar) was used to make a statistical prediction concerning its future trajectory and to improve the chances of hitting the target. The idea of feedback that emerged from the analysis of telephone circuits as well as the control approach of the classical studies on governors (regulation devices, ships steering and so on) found a new field of application in this wartime problem of fire control: these studies formed the basis of modern control and automation engineering, which rests on a highly developed mathematical basis (Bennett 1979–1993). The work of Wiener and Bigelow made a substantial contribution to the development of this branch of technological knowledge as a true form of mathematical engineering: their research led to a statistical theory of prediction based on incomplete information, which rested on previous studies by Wiener on Fourier analysis and ergodic theory (Wiener 1949).

But in addition to the development of control engineering as a research field, the automation of typically human procedures and activities – such as numerical calculation, steering ships, and the aiming of artillery – raised the question of emulation and substitution of the human being with a machine, and developments in this direction quickly arrived. Wiener and Bigelow's studies led to a fruitful exchange of ideas with the Mexican physiologist Arturo Rosenblueth, a professor at Harvard University, concerning the possible analogy between human behaviour and that of machines. The control devices were based on the processing of information – obtained via radar in the case of anti-aircraft artillery fire – by means of a feedback loop that allowed performance to be modified and improved at each cycle. In a joint paper, entitled "Behavior, purpose and teleology", they claimed that such a process was similar to those that take place in the human nervous system and that underlie all organized intelligent behaviour.[145] Wiener was enthusiastic about the project contained in embryo

145) Rosenblueth, Wiener, Bigelow, 1943. The influence of research on automation during the war (radar and servomechanisms) and of control engineering after the war on postwar scientific culture was extraordinary, as is shown for example by the case of the transfer of mathematical tools from gunfire to inventory control (Klein 1999, 2001). Together with the even more evident cultural influence of the development of the computer, it led to the idea of a "cyborg" science in the postwar period which was closely linked to the world view of US military spheres during the Cold War; see for instance Edwards 1996, Pickering 1995, Mirowski 1999 and Galison 1994 (who is concerned with cybernetics). It is beyond our present scope to discuss this literature and it will merely be stated that the influence of military thinking is often over-estimated with respect to the role of progress in scientific thinking due to new ideas of technological (information, control, feedback), mathematical (optimization) or philosophical (system, decision) origin. For example, discussion on operations research in Mirowski 1999 neglects the mathematical content rapidly acquired by this discipline after the war, which makes it possible to link it to ideas on optimization dating back to the Enlightenment;

in this work and came into touch with specialists in different disciplines, including the anthropologist Gregory Bateson and the neuropsychiatrist Warren McCulloch.

McCulloch was looking for a formal model of the neural networks in the human nervous system. In 1943 he published an article written jointly with the young mathematician Walter Pitts, "A logical calculus of the ideas immanent in nervous activity", in which neurons were represented as "boxes" linked together in "networks", inside which impulses or signals governed by logico-mathematical laws were transmitted (McCulloch, Pitts 1943; see Heims 1991). The logical scheme of the article was a combination of Carnap's logical calculus and several ideas taken from Russell and Whitehead's *Principia mathematica*. The authors demonstrated that all behaviour that could be rigorously and unambiguously described using a finite number of words could be performed using one of their formal neural networks. McCulloch and Pitts' model allowed the description of purposive behaviours to be linked to neuropsychiatric and neurophysiological research.

Wiener discussed his new ambitious intellectual project with von Neumann and brought him into touch with the new research environment that was being set up. McCulloch and Pitts' article proved to be of great interest to von Neumann, who was growing familiar in those years with the design and engineering of automated computation machines and with electronics engineering. The basic principle underlying these machines, including the ENIAC calculator, was the same as in the many machines that had succeeded each other over the centuries in the study of artificial calculation; technical progress had increased the speed of computation, but human intervention to regulate the process for each new operation was still indispensable. Von Neumann was convinced of the importance of the new technological devices, but considered that in order to build new, more powerful and efficient machines, it was necessary to develop a theoretical foundation for their structure. Between 1939 and 1941 he had discussed a theoretical analogy between the functioning of the brain and automated calculating devices in an exchange of correspondence with the Hungarian physicist Rudolf Ortvay. McCulloch and Pitts' work provided important support for the development of this analogy.[146]

He illustrated his ideas for the first time in the previously mentioned report on the EDVAC calculator, in which he described a projected fully automated high speed digital calculation system (Neumann (von) 1945a; see Aspray 1990). This consisted of a machine with a large storage memory and an internal command unit to develop the tasks assigned by means of recorded algorithms or *codes* (this was the term in use at the time for what we call today *programs*): this radically innovative project was to open up the way to a modern concept of the electronic computer. Von Neumann considered that such a system or automaton was formally comparable with the human

considering this broader context, the view of operations research as a cyborg science is obsolete. See Kay 2000 for a deep analysis of the conceptual turning point in the life sciences due to the transfer of technological concepts (such as the idea of information underlying the development of molecular biology and genetics).

146) See Aspray 1990, 178. Letters are written in Hungarian; see the English translation in Rédei (ed.) 2005.

nervous system, insofar as it was an information processing system, and since the role of its elements – vacuum tubes, i.e., electronic diodes used as the basic component of future calculators – was analogous to that of the ideal neurons in McCulloch-Pitts' schema.

In his report von Neumann described the five basic elements of the calculator: the central arithmetic unit, the memory unit (which stored both the numerical data and the numerically coded instructions), the central control unit (which controlled the appropriate sequencing of operations and coordinated the functioning of the other units in order to carry out the programmed task) and the input (data entry) and output (emission of results) units. He compared the central unit with associative neurons and the input and output units with the sensory and motor neurons, respectively; he also pointed out the difference between the synchronous regulation of the calculator system, which followed its internal clock, and the asynchronous nature of impulse transmission among neurons.

He in no way neglected the research on technical components of the machine, which were the fruit of the "hardware" engineering design, but he deemed it important to separate this aspect from the logic configuration design. His proposal therefore represented a push towards a further degree of abstraction in electronic engineering. The EDVAC report illustrated indications emerging from von Neumann's interaction with the engineers, and addressed an interlocutor with a technical background, but proposed an approach to the problem "forgetting about" the material components:

> *The ideal procedure would be to treat the elements as what they are intended to be: the vacuum tubes. However, this would necessitate a detailed analysis of specific radio engineering questions at this early stage of discussion, when too many alternatives are still open, to be treated all exhaustively and in detail. Also, the numerous alternative possibilities for arranging arithmetical procedures, logical control, etc., would superpose on the equally numerous possibilities for the choice of types and sizes of vacuum tubes and other circuit elements from the point of view of practical performance, etc. All this would produce an involved and opaque situation in which the preliminary orientation which we are now attempting would be hardly possible.*
>
> *In order to avoid this we will base our considerations on a hypothetical element, which functions essentially like a vacuum tube – e.g., like a triode element with an appropriate associated RLC-circuit – but which can be discussed as an isolated entity, without going into detailed radio frequency electromagnetic considerations. We re-emphasized: This situation is only temporary, only a transient standpoint, to make the present preliminary discussion possible. After the conclusions of the preliminary discussion the elements will have to be reconsidered in their true electromagnetic nature.*[147]

147) Neumann (von) 1945; in Aspray, Burks 1987, 29–30.

Neural networks, electronic systems or the action of genes thus represented examples of an abstract machine of the same type as the famous machine introduced by Alan Turing, whose ideas had influenced both McCulloch and Pitts and von Neumann. Von Neumann had met Turing in 1935, when the latter was still a young student at Cambridge; one year later he met him again at Princeton, where Turing was to complete his studies and his Ph.D. with Alonzo Church, one of the most distinguished mathematical logicians at that university. Mathematical logic continued to develop as an autonomous sector of mathematics, aimed mainly at solving the many problems raised by the crisis of the foundations. Turing was concerned in particular with the solution to the *Entscheidungsproblem* formulated by Hilbert, which had remained open after Gödel's results and that may be summed up as follows: does any procedure exist that allows the truth value ("true" or "false") of any assertion whatever that is formally established in mathematical terms to be determined? This was actually a problem of theoretical computability expressed in constructivist logical terms, similar to the problem posed by the computability of a numerical solution within a reasonable number of steps inside an electronic computer.

In the years 1945 and 1946 von Neumann did considerable work on the principles and implications of modern high speed calculators in collaboration with Goldstine, analyzing the available ideas and developing new ones with a view to future improvements. He gave lectures all over the place in order to disseminate his ideas, both in academic and military circles (Aspray 1990). In a report written together with Goldstine in Spring 1946 for the Applied Mathematics Panel of the NDRC (at the request of the director Warren Weaver), entitled *On the principles of large scale computing machines*, Goldstine and von Neumann wrote:

> *During recent war years a very considerable impetus has been given to applied mathematics in general, and in particular to mathematical physics, particularly in certain important fields which have not been in the past in the focus of most theoreticians' interest. Typical of these fields are various forms of continuum dynamics, classical electrodynamics through hydrodynamics to the theories of elasticity and plasticity. One might also mention various involved problems of statistics* [. . .] *Again, partly under the influence of wartime necessities, but partly also as natural outgrowth of normal industrial development which is turning increasingly towards automating scanning and control procedures, the methods of automatic perception, association, organization and direction have been greatly advanced. These methods were in most cases of the high speed electromechanical, or of the extremely high speed electronic type. Modern radar, fire control and television techniques are good examples of this. These two streams of evolution have produced both an increased need for large scale, high speed, automating computing, and the means, or the potential means, to develop the devices to satisfy this need.* (Goldstine, Neumann 1946, 1)

The breadth of vision implicit in these words, together with the great reputation enjoyed by von Neumann, favoured the acceptance of these ideas by the scientific community. At the same time, he took the necessary steps to organize the construction of an experimental electronic computer in which to apply his ideas.[148] This computer was to be designed in particular for scientific research applications, differing in this from the machines built for the purpose of military applications. Automatic calculation was in its infancy, and was not considered as a scientific activity in the strict sense in academic circles. He therefore went to great lengths to convince his colleagues of the scientific importance of the calculator and received offers from several universities. Wiener, for instance, proposed developing at MIT a research program concerning the engineering and science of control, in collaboration with Harvard. However, many of his mathematical colleagues intervened to have the project carried out at the IAS. In the end, the Institute, despite its tradition as an *élite* centre oriented towards theoretical research in mathematics and physics, decided to back the project, to build the laboratory, and to provide accommodation for the engineers and technical staff: in November 1945, the director of the IAS, Frank Aydelotte, gave his final approval. Aydelotte, in order to convince his reluctant colleagues, compared the future electronic calculator to a large scale telescope.

Preparations for the Electronic Computer Project (ECP) went on for many months. It was necessary to obtain substantial funding, construct the laboratory and set up a suitable scientific and technical group. Von Neumann succeeded in having the ECP turned into a joint venture, with participation of the IAS and Princeton University, of a private company, the Radio Corporation of America (which collaborated in construction of the vacuum tubes), and also of the ONR (which provided support for the related mathematical and meteorological research) and other military services that funded the engineering aspects, the logic project and the programming. The support of institutions with such different goals could have pushed the project in more practical directions. However, von Neumann succeeded in imposing the idea that the objective of the ECP was scientific research in the field of calculation and not just a routine activity.

Von Neumann invited Goldstine to occupy the post of associate director: the latter joined the project in February 1946, after the official inauguration of ENIAC. Together with Goldstine and Arthur Burks, who had collaborated on the ENIAC and EDVAC projects, von Neumann began work on the calculator's logical design: in June 1946 a report was prepared for the AOD – *Preliminary discussion of the logical design of an electronical computing instrument* – in which, by developing the ideas of the EDVAC report, an illustration was given of what is known as the "von Neumann architecture" of a computer, and which even today, regardless of technological progress, still represents the basis of computers.[149] Consideration was given to the conditions of an ideal rapid access memory unit and a description was subsequently

148) On the development of von Neumann's Electronic Computer Project see the thorough reconstruction in Aspray 1990.

149) Neumann (von), Burks, Goldstine 1946 (a second edition of the report dates to September 1947).

given of a differentiation among memory levels, which was dependent on the technology available at the time: a primary memory, implemented by means of the Selectron tube constructed by RCA, the components of which were organized according to a "parallel architecture"; and a secondary memory which organized the data to be transferred to the main memory as required and that were recorded on film or magnetic tape. Subsequently, an examination was made of the functioning of the fundamental arithmetic operations: the representation of the numbers inside the machine was binary, and a fixed point notation was used (floating point representation used today was introduced later). Several aspects were neglected and many ideas were examined in a non-definitive way, as it was necessary to await the study and implementation of many technical elements.

The same month of June saw the arrival at the IAS of Bigelow, who had accepted the post of chief project engineer, and who had a group of six electrical engineers and several technicians working under him. The work of organizing the laboratory extended into the first few months of 1947; then came implementation of the technical aspects, such as the memory components, the input and output units, etc. The three years envisaged for the construction became six: there was no time to carry out trials of the experimental machine and then to construct the definitive version, and so the prototype was perfected. Many specialists visited the ECP and several US and foreign institutions were authorized to construct copies, such as the JOHNIAC built by the RAND and the MANIAC built at Los Alamos Laboratory.

At the end of 1950 work began on processing the first test problems and in the summer of 1951 the first significant real problem was processed, at the proposal of the Los Alamos group, and required a computing time of several hundred hours. Finally, on 10 June 1952, a reception was held to inaugurate the computer. The computer working time gradually increased with respect to the time required for overhaul and checks. The project continued until 1957, when the machine was donated to the Princeton University, which continued to use it for another three years. In the meantime, technology had been developing rapidly: transistors had replaced the vacuum tubes, thus allowing a huge reduction in calculator size and an increase in computing power. Private companies such as IBM and UNIVAC began to conquer the civilian and military calculator building market. Von Neumann's computer was already literally a museum piece: several parts of the machine are now conserved at the National Museum of American History in Washington, D.C.

As Aspray (1990, 235) remarks, von Neumann's work marked the take off of computer science and technology in the United States in the first decade of the postwar period:

> [...] *most senior members of the American scientific community who had an interest in computing, such as Vannevar Bush and Samuel Caldwell, were wedded to the analog technology. Von Neumann was unusual among the scientific elite for his knowledge of and dedication to digital computing technology. His international scientific stature gave credence to the computer as a technology worthy of scientific attention. His contacts*

> *at high levels in the federal government, particularly within the military community, opened up channels of funding for computer purchase and development. He was in great demand as a lecturer and adviser to the scientific societies, industry, and federal government, and everywhere he went, he preached about the importance of the digital computer.*

In the 1960s and 1970s there was an acceleration in the development of computer science or, more precisely, of information science and technology broadly conceived as a set of highly effective and sophisticated concepts and techniques for the automatic processing of information. The computer ceased to be a tool exclusive to the military services or the great universities, and began to be massively used in industrial production (in symbiosis with automation technology) and in managing many civil systems (in symbiosis with communications technology). At a later stage, favoured by the gradual miniaturization of electronic components, the big systems gave way to the birth of microcomputing and the spread of information technology among an ever-expanding number of users of personal computers, all of them connected to the Internet. This was a development that, according to what von Neumann's daughter Marina Whitman claims, her father would have found it hard to imagine:

> [...] *I don't think he was a very accurate prophet regarding what turns the practical applications of his pioneering work would take. For example, he clearly expected that the computer would have its impact primarily on scientific research and military work. He was particularly interested in its role in advancing the accuracy of weather forecasting and, ultimately, climate modification. I don't think progress in this area has been nearly as far or as fast as he hoped and expected. Similarly, I think he anticipated that the theory of games would have more impact* [...] *on poker, business, and war than it has turned out to have had so far, at least.*
>
> *On the other hand, if anyone had ever told him that the company I work for, General Motors, would produce and utilize literally millions of computers every year* (*each of the roughly eight million vehicles we produce each year contains several, not to mention the ones in our plants and offices*)*, I think he would have been startled. And the notion of adults fulminating against computers as corrupters of youth in the form of video games would have amused and perhaps secretly pleased the playful, child-like aspects of his personality.*[150]

150) Glimm, Impagliazzo, Singer (eds.) 1990, 2–3.

5.5 The use of computer in scientific research

The mathematical analysis of a scientific problem is a phase that is quite distinct from its numerical analysis. For example, if we take a problem of mathematical physics expressed by means of a differential equation, the *mathematical analysis* of the equation consists in seeking to demonstrate the existence of solutions and their possible uniqueness; and then in the attempt to solve the equation, or to obtain an explicit expression of the solutions, which is rarely possible. In many cases all that can be done is to simply study the properties of the solutions and above all their "qualitative" structure (i.e., their geometric behaviour). *Numerical analysis*, on the other hand, consists in computing the numerical values of the solutions corresponding to a specific numerical choice, not only of the equation coefficients, but also of the parameters defining the initial physical state of the system. This second phase represents a natural development of the first if an explicit formula of the solution is known: by substituting the specific numerical values of the coefficients and of the parameters, the desired "numbers" are obtained. However, as it is practically never possible to obtain a formula that explicitly expresses the solution, quite frequently, mathematical analysis stops short of a demonstration of existence of a solution.

In modern mathematics, prior to the development of automatic computing, the relation of dependence of numerical analysis on mathematical analysis led to the power of analytical methods being constantly extended – that is, tools for explicitly expressing solutions, perhaps using more complex functions than those of elementary analysis – so as to make numerical solutions possible. But even when it was not possible to obtain explicit solutions, mathematicians attempted to devise constructive numerical calculation procedures for specific cases, given by sequences and iteration of operations, the "recipe" for which goes by the name of *algorithm*. The construction of algorithms for the direct numerical solution of a mathematical problem has always been at the focus of mathematicians' concerns, from Newton to Gauss and many others.

An algorithm for the numerical solution of a differential equation can sometimes allow it to be "solved" approximately (and, of course, for particular values), even when mathematical analysis is unable to proceed beyond the mere demonstration of the existence of a solution. In this sense, numerical analysis, although linked to mathematical analysis, has several forms of autonomy. However, only in the twentieth century did it become a mathematical discipline in its own right, which is concerned with the description of general solution methods and their evaluation through the estimation of errors and of their propagation and through the study of stability and efficiency (with the introduction of *ad hoc* definitions and criteria). Starting in the 1920s, a new phase began which quickly rendered obsolete the nineteenth century graphic methods of numerical solution, above all thanks to the research carried out in Germany in view of applications, including those of Carl Runge and Richard Courant, both professors at the University of Göttingen.[151] However, the development of the

151) Runge, König 1924. Von Neumann's principal collaborator in this area was the author of a history of numerical analysis until 1900 (Goldstine 1977).

methods and power of numerical analysis received an extraordinary impetus after World War II and in connection with use of electronic calculators.

Precisely the knowledge of the new ideas developed in Germany before the war must have helped von Neumann to understand the relevance of automating calculation in the development of numerical analysis and therefore, for the solution of hitherto inaccessible applications problems. In the above-mentioned 1946 work *On the principles of large scale computing machines*, Goldstine and von Neumann focused on problems that give rise to non-linear ordinary or partial differential equations. Until that time, they argued, physics and mathematical physics had concentrated almost exclusively on the study of linear equations (those in which only linear functions are involved). The authors emphasized the limit that this could set for the development of applications of mathematics, which almost always have to cope with non-linear problems:

> *A brief survey of almost any of the really elegant or widely applicable work, and indeed of most of the successful work in both pure and applied mathematics suffices to show that it deals in the main with linear problems. In pure mathematics we need only to look at the theories of partial differential equations and integral equations, while in applied mathematics we may refer to acoustics, electrodynamics, and quantum mechanics. The advance of analysis is, at this moment, stagnant along the entire front of non-linear problems. That this phenomenon is not of a transient nature but that we are up against an important conceptual difficulty is clear from the fact that, although the main mathematical difficulties in fluid dynamics have been known since the time of Riemann and of Reynolds, and although as brilliant a mathematical physicist as Rayleigh has spent the major part of this life's effort in combating them, yet no decisive progress has been made against them – indeed hardly any progress which could be rated as important by the criteria that are applied in other, more successful (linear!) parts of mathematical physics.*
>
> *It is, nevertheless, equally clear that the difficulties of these subjects tend to obscure the great physical and mathematical regularities that do exist.*[152]

They went on claiming that the development of computing due to the use of numerical analysis methods in high speed calculators would open up new horizons in mathematics, not only with reference to non-linear problems but also in the solution of large size numerical problems. During the early years of the development of the ECP, when the future electronic machine was still at the drawing board stage, Goldstine and von Neumann carried out intense theoretical work intended to foster future use of the computer in scientific research. On the one hand, they developed a wide-ranging study of numerical analysis methods; on the other hand, they worked on the

152) Neumann (von), Goldstine 1946, in JNCW, vol. 5, 2–3.

programming or "coding" techniques so that numerical problems could be processed using the computer.

Between 1947 and 1948 Goldstine and von Neumann published a three-part report entitled *Planning and coding problems for an electronic computing instrument* which represented a continuation of the preceding report written together with Burks.[153] The authors emphasized the noticeable differences that existed between the mathematical formulation of a problem and its coding in the form of instructions that the computer is called upon to execute. The problem they faced was not one of simple translation but rather how to come up with a "control scheme for a highly dynamical process"[154] in which the orders do not follow in linear sequence but in an essentially dynamic fashion that allows forward and backward movements in the sequence of instructions or allows appropriate substitutions to be made in the sequences as a result of given intermediate results obtained during the computing process. This idea took shape in a new logical tool, the "flow diagram", which consists of a graphical representation of processes taking place inside the calculator, including closed cycles ("loops") and recursive indexes.

It should be noted that the terms used by the authors (control, dynamical process, flow) belonged to the technical-mathematical language that was in full swing in those years; as we have already pointed out, on the part of the electronics and communications engineers this language reflected a new conception of the technical configurations in global (or system) and dynamic (or process) terms. Flow diagrams and flow charts quickly became familiar conceptual tools in both engineering and coding/programming handbooks, as well as in the organizational analyses of factory flows in scientific management books. For programming –as before for the configuration of the computer – von Neumann strongly supported the trend towards an abstract approach, without consideration of specific context of application:

> *Since coding is not a static process of translation, but rather the technique of providing a dynamic background to control the automatic evolution of a meaning, it has to be viewed as a logical problem and one that represents a new branch of formal logics.*[155]

Furthermore, *Planning and coding problems* presented programming methods that were completely different from those used in preceding calculators, in which coding was repeated *ex novo* each time a problem was programmed. The new approach consisted in storing "subroutines" of frequently used programs so that they could be incorporated in larger programs whenever necessary. As Aspray points out (1990, 69), although Goldstine and von Neumann's work consisted only of a preliminary report, it was widely circulated in the United States and Europe and represented a fundamental reference source for many years.

153) Goldstine, Neumann (von) 1947–48. Together with the already mentioned Neumann (von), Burks, Goldstine 1946, they formed the report to AOD under contract W-36–034-ORD-7481.

154) Ibid., in Aspray, Burks (eds.) 1987, 155.

155) Ibid., in Aspray, Burks (eds.)1987, 154.

The use of high speed computing systems led to problems that were very different from those encountered not only, of course, in manual calculation, but also in the use of the first calculating machines: it was thus necessary to find a new orientation for numerical analysis in order to adapt it to the needs of programming an electronic computer.[156] In 1947, Goldstine and von Neumann published in the *Bulletin of the American Mathematical Society* an article devoted to the numerical inverting of matrices of high order: in the article an examination was made from the new angle of the Gaussian elimination method, which is one of the mainstays of numerical analysis. The second part of the article, in which the error was estimated using probabilistic methods, was published in 1951.[157] The authors identified several fundamental issues and offered a critical discussion of current research in this area of mathematics. In order to improve the efficiency of a numerical algorithm, it was necessary to take into account its numerical stability, that is, the property whereby the accumulation of the inevitable rounding off errors in a large number of operations ("noise") is not amplified, thus producing serious errors in the results; and it was necessary to take into account its complexity, that is, the duration and number of computations necessary, which might make calculation impracticable in the case of large-scale problems.

In the years that followed, von Neumann worked on numerical problems in many different environments. The ENIAC calculator, which was inaugurated in 1946, set up in the Aberdeen Laboratory and then reconverted for use as a programmable machine, was used to handle many of these problems while the Princeton calculator was being built. After the latter was inaugurated, the ECP team directed by von Neumann began systematically to develop the theoretical study of numerical methods – for example, the Runge-Kutta method for the solution of ordinary differential equations was identified as the best possible in terms of efficiency and error control. At the same time, numerical analysis was performed on scientific problems submitted by researchers, in particular from Princeton, or by government laboratories, all appropriately selected. The work of numerical analysis, programming, and the computing time were paid for by the users, but the Princeton team often managed to find funds to cover these costs in the case of individuals. This activity touched upon problems related to number theory, astrophysics, fluid dynamics, atomic and nuclear physics, and many other areas, thus allowing the theoretical knowledge of the methods of numerical analysis to be improved, in the spirit that had stimulated the project right from the outset.

Von Neumann had emphasized that one of the main fields in which computer-assisted numerical analysis was called upon to provide important results was the numerical solution of partial derivative equations in fluid dynamics. This kind of problem was found in fields such as atomic energy, aeronautical engineering, oil prospecting and weather forecasting. Von Neumann and his collaborators and colleagues at the IAS and Los Alamos devoted careful attention to these problems. Von Neumann's contributions in this field include study of the numerical stability of fi-

156) On von Neumann's contribution to the history of numerical analysis, see Aspray 1990, Chapter 5.

157) Goldstine, Neumann (von) 1947, 1951.

nite difference methods based on the pioneering research of Courant, Kurt Friedrichs and Hans Lewy, published in 1928; or the study of "shocks", a phenomenon that is found in the study of the supersonic flow of a compressible fluid, which consists in a discontinuity of the density, temperature or of some other physical variable. In the following years this research was gradually extended to cases of increasing scientific importance as computers increased in power; nevertheless, it was the IAS computer that initiated this line of development, which helped to show the potential benefit of scientific computing to the scientific community.

As illustrated above, ever since his arrival in the Los Alamos Laboratory during the war, von Neumann had been concerned with the application of computing machines in research on the atomic bomb. The first calculation performed by ENIAC, after being tested, was a mathematical model that asked for the solution of a system of three partial differential equations. The first calculation performed by the IAS calculator was again one commissioned by Los Alamos. As part of the research developed at Los Alamos, von Neumann, in collaboration with Ulam, developed a radically innovative numerical method, compared with classical methods, based essentially on the discretization of the problems. This was the Monte Carlo method, which they had developed to provide a stochastic model of the problem of neutron diffusion. The behaviour of neutrons is described by certain integro-differential equations that are practically impossible to solve when millions of neutrons are involved that moreover seem to behave in a completely random fashion. Not only the theoretical resources but also the experimental techniques proved insufficient in the study of this phenomenon: Ulam's idea, developed through a systematic collaboration with von Neumann, was to use random numbers that could be produced by the computer with a certain statistical distribution, in order to simulate the time evolution of the neutrons' characteristics and behavior. The Monte Carlo method was later applied both to probabilistic type problems and to problems formulated in strictly deterministic terms in the fields of atomic physics, statistical mechanics or of materials resistance (in particular, in aeronautical applications). After this research, von Neumann took an interest in the question of the generation of random or "pseudo-random" numbers using calculators.

Nevertheless, the most ambitious and fascinating project developed by von Neumann, and the one he considered to be truly symbolic of the meaning of application of computers to scientific research, was the meteorological studies developed in the ECP right from the outset. This research was based on sophisticated mathematical models and had as its final objective an understanding of geophysical dynamics and of the atmosphere, but it could achieve its objective only by providing numerical data and reliable forecasts. Without doubt, the very large number of variables involved and the high complexity of the problem seemed to indicate that only the use of a computer could allow the problem to be tackled with any chance of success. As Aspray has pointed out,[158] until World War II, weather forecasting was dominated by a subjective method based on interpretation by a meteorologist of available data and

158) On the origins of modern weather forecast modelling and use of the computer see Aspray 1990, Chapter 6.

weather charts – by means of an extrapolation of the main pressure systems, drawing on personal experience and on a vast collection of maps of various meteorological profiles, as well as on certain theoretical knowledge of the physical phenomena involved. Thus, weather forecasting was considered more of an art than a science. And yet this did not stop governments from investing increasing amounts of money to fund meteorological studies, to train weather experts and to construct observatories and data recording centres. Indeed the two world wars demonstrated the importance of weather forecasts in military operations: suffice it to think of the effect weather forecasts had on decisions concerning the Allied landing in Normandy in 1944.

In the late nineteenth century, investigations were undertaken that, although not having any great influence on specialists in the field, nevertheless aimed at mathematizing meteorology and turning it into an "exact science" based on a general theory of atmospheric phenomena. Several scientists had considered the possibility of applying the laws of hydrodynamics to the atmosphere. Among others, they included Lord Kelvin, Lord Rayleigh, Hermann Helmholtz and his pupil, Heinrich Hertz. Nevertheless, the first attempt to provide a global description of atmospheric dynamics was the work of the Norwegian scientist Vilhelm Bjerknes, who in 1904 claimed that weather forecasting could be improved only by elaborating a description based on the laws of mechanics and physics, as had happened centuries earlier with astronomy. However, mathematical analysis did not possess any methods for resolving the system of partial differential equations on which such a description was based, and the numerical methods were impracticable owing to the large number of calculations required. Bjerknes used graphical methods and differential geometry and later the British meteorologist Lewis Fry Richardson, further developing Bjerknes' approach, applied finite difference approximation methods to meteorology equations

In Richardson's *Weather prediction by numerical process* (1922), a book which aroused considerable attention, the author explained the reasons for the considerable degree of error that could affect twenty-four hour weather forecasting. From a practical point of view, the problem of effective weather forecasting remained open because of the large number of calculations required: Richardson had estimated that weather forecasting over the entire world would have required 64,000 human calculators. The new technical inventions applied to data determination, such as radiosondes, radar, rockets and, more generally, the development of communications, seemed to hold significant promise of increased accuracy. Progress in the automated processing of numerical data could allow further advantages: this idea was developed in the months following the end of World War II by the RCA engineer Vladimir K. Zworykin – the father of television – in connection with his work at the ECP. Conversations between the latter, von Neumann, Francis W. Reichelderfer (director of the US national weather service), and Carl-Gustav Rossby (one of the most distinguished American meteorologists) led to the decision to include meteorological research in the ECP. Indeed von Neumann was not interested so much in producing weather forecasts as in the study of theoretical problems that could be implemented using the computer under construction and later applied to prediction and even to the modification of weather. For this purpose a meteorology group was set up in the ECP, funded by the

Navy and by the ONR, who was presented to the meteorologists' community during a conference organized by von Neumann at Princeton in August 1946.

The research group was coordinated by Jule Charney, who joined it in 1948; Norman Phillips began collaborating in 1951. Setting out with the aim of improving Richardson's results, the group developed two and three-dimensional models of the atmosphere (the barotropic model for the conventional 24 hour forecast and the baroclinic model designed for storm weather forecasts), which were tested by application to various cases of weather forecasting. In 1953 the theoretical and numerical aspects of the general circulation in the atmosphere were addressed. At this stage, the civilian weather service and various military organizations began to take an interest in the practical application to weather forecasting of the models developed by the ECP. Von Neumann himself made efforts to stimulate this interest, promoting action by his team to develop operating forecasting procedures and the training of staff. Collaboration among the various government meteorological services culminated in the establishment in 1954 of the Joint Numerical Weather Prediction Unit. Von Neumann also played an important role the following year in organization of the General Circulation Research Section, which was also funded by these services, for the purpose of developing the line of research begun at IAS.

As Heims (1980, 152–153) has pointed out, von Neumann's approach to the general problem of meteorology was a good indication of his mature scientific philosophy, especially compared with the approach proposed by Wiener to tackle the same problem. Von Neumann was deeply convinced of the validity and effectiveness of classical, strictly deterministic meteorology. In other words, he pursued an analytical description of the dynamics of the atmosphere that would provide valid forecasts by means of suitable approximations and numerical techniques assisted by electronic computer: it was a matter of improving information processing techniques in a struggle against time in order to obtain reliable 24 hour weather forecasts. In other words, it was a matter of coping with high complication, and not with intrinsic complexity. Wiener, on the other hand, criticized this approach, emphasizing that the meteorologist's information concerning the state of the atmosphere is necessarily incomplete, and he thus considered it to be more suitable to state the problem of forecasting in statistical terms. As we have seen in other circumstances and as we will see in the following section, von Neumann by no means attempted to duck the probabilistic-statistical implications, although he certainly subordinated them to the classical approach on which he had based his meteorological analysis project.

In 1955, thinking of his future scientific work beyond his term of office at the AEC, von Neumann planned to take Charney and Phillips with him to the University of California, where he hoped to develop geophysical and meteorological research (both of them then joined MIT). In addition to this important scientific application of computing, and in view of the fact that technological development and spreading use of computers around the country had now become a commercial issue under the control of private enterprise, his attention was now turned towards the logical and theoretical aspects posed for a mathematician by the new "electronic brains".

5.6 The brain-computer analogy

A convergence of disciplines had taken place in the 1940s and 1950s around the idea of *algorithm*: logic, which was concerned with problems of theoretical computability; the brand new computer science; and numerical analysis and the various branches of applied mathematics. Subsequently, numerical analysis – following a trend analogous to that in other branches of mathematics – detached itself from the foundational problems arising out of this convergence of disciplines.

The computer, as was becoming increasingly obvious, was not only a machine for performing numerical calculations much faster than before, but a device capable of automatically processing information in a much broader sense. Together with other technological devices – radar, the telephone and feedback control mechanisms – it would allow practically every kind of human activity to be automated. To this end it was however necessary to have a thorough knowledge of these inventions, of the interaction among them in systems and networks and of the control structures needed to ensure their automatic functioning. *Computer science* (the science concerned with the machines) was thus to evolve towards *information science* (the science concerned with an immaterial entity linked to the intelligence of man himself). Consequently, the relationship between logic and information science became even closer. Not only that: involved in this convergence of disciplines were also some sectors of the life sciences, such as neurophysiology and molecular biology, the developments in which were followed by many physicists and mathematicians with great attention.

Von Neumann was indubitably one of the masterminds behind this symbiosis. He was in fact increasingly less involved in the technical aspects of computer design and the problems of numerical analysis and concentrated on the more specifically logical aspects: in this way he closed an ideal circle in his career as a scientist. In Chapters 1 and 2 we have described the first mathematical issue that attracted his intellectual curiosity, set theory, which lies on the boundary between logic and mathematics, and his subsequent interest in mathematical logic issues within the framework of the Hilbertian formalist program. It was stressed how these studies left a deep impression on the way von Neumann tackled many different scientific problems in the course of his prolific career, an impression that is reflected in his complete acceptance of the axiomatic approach and in his concern always to seek a "logically adequate formulation" of the problems.

These two aspects – the axiomatic approach and the identification of the underlying "logic" of the problem – are inseparable in his conception: a very clear example of this comes from his quantum mechanics research. After finishing his work on the axiomatic formulation and mathematical foundation of quantum mechanics, he went on to study the underlying logic of this theory in an important article published in 1936 together with Garrett Birkhoff: in the article they studied the logical implications of quantum mechanics and set out to propose an extended, "probabilistic" logic that was to the new mechanics as classical propositional calculus was to classical mechanics (Birkhoff, Neumann (von) 1936). The new propositional calculus introduced here was equivalent to the calculus of linear subspaces in an abstract projective

geometry and made use of the lattice theory developed by Birkhoff. This "logically adequate formulation" thus represented for von Neumann a fundamental tool for addressing the emergence of probabilistic and random aspects in twentieth century science. Moreover, as Miklós Rédei has shown, von Neumann's research of the best mathematical formalism for quantum mechanics was guided by his understanding of the "underlying logic" of any scientific problem:

> *The attitude of the physics community towards von Neumann's work has remained ambivalent: while von Neumann's work is appreciated as an outstanding intellectual achievement, in fact his work is looked upon by physicists as a luxury, as an example of striving for mathematical exactness for its own sake. On the other hand, the precise content of* (*and physical motivation for*) *von Neumann's admittedly rather mathematical results seems to be not widely known.* [...] *The moral of this story of von Neumann's intellectual move from the Hilbert space formalism towards type II* (*and even more general*) *algebras is that what drove him was not the desire to have a mathematically unobjectionable theory – there was nothing wrong with Hilbert space formalism as a mathematical theory. What von Neumann wanted was conceptual understanding. He was ready to leave behind any mathematical theory – however beautiful in itself – to achieve that.* (Rédei 1996)

The idea of extending logic was subsequently applied by von Neumann in his studies on calculators and on artificial automata in general. It appears from the outset in his first discussion on the topic, dating back to the late 1940s:

> *We have emphasized how the complication is limited in artificial automata, that is, the complication which can be handled without extreme difficulties* (*and for which automata can still be expected to function reliably*). *Two reasons that put a limit on complication in this sense have already been given. They are the large size and the limited reliability of the componentry that we must use, both of them due to the fact that we are employing materials which seem to be quite satisfactory in simpler applications, but marginal and inferior to the natural ones in this highly complex application. There is, however, a third important limiting factor, and we should now turn our attention to it. This factor is of an intellectual, and not a physical, character.*
>
> The limitation which is due to the lack of a logical theory of automata. *We are very far from possessing a theory of automata which deserves that name, that is, a true mathematical-logical theory. There exists today a very elaborate system of formal logic, and, specifically, of logic as applied to mathematics. This is a discipline with many good sides, but also with certain serious weaknesses. This is not the occasion to enlarge upon the good sides, which I have certainly no intention to belittle. About the*

> *inadequacies, however, this may be said: Everybody who has worked in formal logic will confirm that it is one of the technically most refractory parts of mathematics. The reason for this is that it deals with rigid, all-or-none concepts, and has very little contact with the continuous concept of real or of the complex number, that is, with mathematical analysis. Yet analysis is the most successful and best-elaborated part of mathematics. Thus formal logic is, by the nature of its approach, cut off from the best cultivated portions of mathematics, and forced onto the most difficult part of the mathematical terrain, into combinatorics.* (Neumann (von) 1951, in JNCW, vol. 5, 302–303)

There were two fundamental aspects in the foundation of a theory of artificial automata in von Neumann's opinion. In the first place, the fact that a key criterion of formal logic, that is, whether a result may be obtained or not in a finite number of elementary steps, had to give way to consideration of the actual length of the chain of reasoning or – with reference to the construction of an automaton – of the chain of operations. In the second place, it was necessary to introduce aspects of uncertainty or of probability, so that logical operations – or their equivalents in the actions of automata – would be treated using processes that allowed exceptions (dysfunctions or errors) with non-zero probability. The key for extending logic might be found, claimed von Neumann, in «thermodynamics, primarily in the form it was received from Boltzmann, and is that part of theoretical physics which comes nearest in some of its aspects to manipulating and measuring information. Its techniques are indeed much more analytical than combinatorial [. . .]» (Ibidem, 304).

In the study of automata, the definition of the concept of information by means of its statistical mechanical properties was thus called upon to play the important role assigned to it by Claude Shannon in the mathematical theory of communication: essentially, also in the case of automata, we are dealing with a problem of communication and information transmission subject to a certain "noise" that could alter it. Shannon had formulated an abstract definition of the elements of a communication system (channel, source, emitter, destination, receiver) and had introduced the theoretical idea of information as a measure of the degree of freedom of the emitter when it selects a message – distinguishing it from the customary concept of "quantity" of meaning contained in a message. Shannon's ideas, published in 1948 in the "Bell System Technical Journal" and presented in systematic form the following year in the already mentioned, renowned book *The mathematical theory of communication*, were widely circulated and led to a vast range of scientific and technical developments in the second half of the twentieth century.

From the formal point of view, a mathematical formula expressing the information content of a system coincides with the definition of entropy of a physical system in statistical mechanics. It was von Neumann who pointed out to Shannon that the connection between entropy and "lack of information" or negative information of a system had been introduced by Ludwig Boltzmann in 1894:[159] entropy was related to the number of alternatives available to a physical system after all the macroscopically

observable information had been recorded. Leo Szilard had studied this connection in order to resolve the famous physical paradox of Maxwell's "demon", an intelligent being capable of defying the second law of thermodynamics: the paradox was resolved by taking account of the fact that the loss of entropy in a system corresponded to an increase in information; in this way, entropy could be considered as the expression of the quantity of uncertainty or randomness of a system (Szilard 1929). Von Neumann himself subsequently applied Szilard's results to the problem of information in the context of quantum mechanics.

The passage from von Neumann quoted at the beginning of this section is part of a lecture held in September 1948 at Pasadena in the course of the Hixon Symposium on cerebral mechanisms in behaviour, entitled "The logic of analogue nets and automata" and published in 1951 in the Symposium proceedings (Neumann (von) 1951). This was the first article published by von Neumann on the theory of automata even though his thinking on this subject had begun in the course of his work on electronic computers. Indeed the ideas he presented at Pasadena had already been largely illustrated in June 1946 at Princeton during an informal seminar. We have already seen how, right from his first expositions of the logical project of a modern computer, he had made use of a comparison with the brain. His project, as he described it at Princeton and Pasadena, was precisely that of using this comparison as the basis of the construction of a general, logical theory that would encompass both the brain and the computer, although taking into account – as he said in the passage quoted above – the inferiority of artificial materials compared with natural ones "in this highly complex application". It was a matter of developing a theory capable of describing the organization and dynamics of perceptive and cognitive processes; a theory embracing the whole range of actual processes involved in the treatment of information both in live beings and in electronic computers and automatic devices. Therefore, by means of an extension of formal logic, a theory of automata should include also error, propositions known only with a given probability, and similar phenomena not dealt with in classical formal logic.

The background against which the theory of automata developed was actually that of intensive contacts with scientists from the biomedical environment, above all, neurophysiologists. We have mentioned that the functioning of the human brain had been the subject of an exchange of letters between von Neumann and his friend and colleague Ortvay, a professor at the Pazmany Peter University of Budapest in the years 1939–1941.[160] Ortvay was interested in the consequences that quantum theory could have in this environment and von Neumann wanted to examine more closely the role of the observer in quantum mechanics insofar as it was a "quasi-physiological"

159) From Aspray's reconstruction (1990, 174 ff.), it convincingly emerges that the interaction with Shannon played an important role in the latter's ideas, even though he points out (312, note 7) that he was unable to get Shannon to provide evidence in this sense. Shannon's scanty knowledge of the historical precedents provides evidence of the cultural breakdown represented by the Bell Laboratory research.

160) See Aspray 1990, 178 ff. A group of selected letters from von Neumann to Ortvay translated into English can be found in Rédei (ed.) 2005.

concept. In their correspondence, Ortvay set out a large number of ideas on the brain and, in particular, a conception of it as a “switching system” comprising a network, the nodes of which are the cells through which the impulses are transmitted; he also referred explicitly to the differences and similarities with electronic computing systems and with other technical units such as automatic telephone centres and radio installations. Ortvay and von Neumann agreed on the need to identify a theoretical nucleus that would allow several simple basic properties to be identified inside the overall complexity of the problem. Ortvay held that only with a mathematician’s or physicist’s mind – and not with that of the physician or physiologist – would it be possible to attain the aim of axiomatizing complex organized systems.

We have already seen how similar ideas aroused great enthusiasm in Wiener in the 1940s. Towards the end of 1944, with the war in full swing, Wiener, with Howard Aiken and von Neumann, organized a select meeting dedicated to the topic of information processing, inviting a small number of specialists in communication engineering, computers and neurophysiology. Many of these experts were working on secret military projects and for this reason no public invitations were made. In the meeting, which was held in January 1945, von Neumann gave an exposition on computing machines, Wiener one on communication engineering, McCulloch and the Spanish born neurophysiologist Rafael Lorente de No on the organization of the brain. The intention of the organizers was to plan the creation of a new branch of learning equipped with suitable working tools, such as a scientific society and a journal, and to find the funds required for research.

The only initiative that finally saw the light of day, thanks to the support of the Josiah Macy Jr. Foundation, was the organization of a series of conferences in New York on “Circular Causal and Feedback Mechanisms in Biological and Social Systems”. The first of these conferences was held in March 1946 and was chaired by McCulloch: it was practically a repetition of the meeting held in January 1945, but was also attended by a group of representatives of the human sciences, including the anthropologists Gregory Bateson and Margaret Mead, the sociologists Lawrence K. Frank and Paul Lazarsfeld, the psychologists Molly Harrower and Heinrich Klüver, and representatives of other branches of learning (such as George Evelyn Hutchinson, a well-known specialist in ecology), who communicated their views on the consequences of the new ideas in their own disciplines. In the years that followed, and until 1953, ten such meetings were held, always in the initial interdisciplinary spirit: von Neumann attended them and his addresses always had an extraordinarily preeminent influence (Aspray 1990, Heims 1991). Nevertheless, with all his interests and many concerns, his commitment to this project was not particularly intense: the truly charismatic figure was without doubt Wiener, who christened the project with the name of *cybernetics* (from the Greek *kybernétes*, “helmsman”) and in 1948 published the famous book that became the manifesto of the group: *Cybernetics: Control and communication in the animal and the machine.* In it Wiener set out the mathematical and philosophical foundations of active intentional behaviour (in which a “piloting” or control process occurs): in this study a crucial role is played by the idea of *feedback*. Here are some comments by von Neumann concerning Wiener’s ideas:

> *The book's leading theme is the role of feedback mechanisms in purposive and control functions.* [...] *Several students of the subject will feel that the importance of this particular phase of automat-organization has been over-emphasized by the author.* [...] *The reviewer is inclined to take exception to the mathematical discussion of certain form of randomness in the third chapter of the book.* [...] *The technically well-equipped reader is advised to consult at this point some additional literature, primarily L. Szilard's work.* [...] *There is reason to believe that the general degeneration laws, which hold when entropy is used as a measure of the hierarchic position of energy, have valid analogs when entropy is used as a measure of information. On this basis one may suspect the existence of connections between thermodynamics and the new extensions of logics.* (Neumann (von) 1949a, 33–34)

In a letter to Wiener written three years before, von Neumann expressed his scepticism toward a neurophysiological approach to the study of the brain. His observations are particularly interesting:

> *What seems worth emphasizing to me is, however, that after the great positive contribution of Turing-cum-Pitts-and-McCulloch is assimilated, the situation is rather worse than before. Indeed these authors have demonstrated in absolute and hopeless generality, that anything and everything Brouwerian can be done by an appropriate mechanism, and specifically by a neural mechanism – and that even one, definite mechanism can be "universal". Inverting the argument: Nothing that we may know or learn about the functioning of the organism can give, without "microscopic", cytological work any clues regarding the further details of the neural mechanism.* [...] *I think you will feel with me the type of frustration that I am trying to express.* [...]
>
> *To understand the brain with neurological methods seems to me about as hopeful as to want to understand the ENIAC with no instrument at one's disposal that is smaller than about* 2 *feet across its critical organs, with no methods of intervention more delicate than playing with a fire hose* (*although one might fill it with kerosene or nitroglycerine instead of water*) *or dropping cobblestones into the circuit.* [...] *And our intellectual possibilities relatively to it are about as good as some bodies vis-a-vis the ENIAC, if he has never heard of any part of arithmetics.*
>
> *To be more par terre: Consider, in any field of technology, the state of affairs which is characterized by the development of highly complex "standard components", which are at the same time individualized, well suited to mass production, and* (*in spite of their "standard" character*) *well suited to purposive differentiation. This is clearly a late, highly developed style, and not the ideal one for a first approach of an outsider to the subject, for an effort toward understanding. For the purpose of understanding*

> *the subject, it is much better to study an earlier phase of its evolution, preceding the development of this high standardization-with-differentiation, i.e., to study a phase in which these "elegant" components do not yet appear. This is especially true, if there is no reason to suspect already in that archaic stage mechanisms* (*or organisms*) *which exhibit the most specific traits of the simplest representatives of the above mentioned "late" stage.*[161]

Von Neumann also took an interest in the studies directed by the biophysicist Max Delbrück on the bacteriophage. In the same letter to Wiener in November 1946 he claimed that he considered this study of a simple organism – although complex enough to present the typical behaviour of information treatment that lay at the focus of the theoretical interest – more promising from the point of view of a possible mathematical study than the extremely complex study of the human nervous system. Moreover he also described the exciting prospects facing experimental studies carried out using electron microscopes and X-ray analysis. In this connection, it should be pointed out that in 1947 he presented to the ONR a project to set up a protein study group composed of engineers, physical chemists and crystallographers. The idea of the project was rejected and von Neumann stopped pursuing these ideas. The biographer may perhaps be legitimately asked how von Neumann could have included a project of this kind in a professional life that was already so full and complicated: in any case this was not an exception, as quite a few of his physicist colleagues displayed a growing interest in biology – often with the spirit of Ortvay's view – including Szilard, Gamow and Schrödinger, author of the famous book *What is life?* (1944).

It should be noted that Delbrück's project gathered together a group of researchers that was later to succeed in discovering the structure of DNA, thus giving rise to molecular biology. It should also be recalled that, as shown by Lily Kay, between the late 1940s and the early 1950s, that is, just before the discovery of the double helix structure of DNA in 1953, the notion of the "organism and molecules as information storage and retrieval systems, and heredity as information transfer"[162] spread to many sectors of the life sciences and accompanied the growth of molecular biology around the notion of interaction between DNA and proteins in terms of "genetic code". In this connection Kay recalls the influence of the ideas of Wiener, von Neumann and Shannon. However, Heims has emphasized the differences between the approach of Wiener and the supporters of the cybernetic view and the approach of Delbrück and the pioneers of molecular biology.[163] The former were actually seeking an interdisciplinary approach involving common points displayed by phenomena in different spheres (human, biological, artificial), while the latter followed an ultrareductionist programme, namely one involving a search for the physical bases of inheritance through the study of cellular metabolism, itself reduced to analysis of

161) Neumann (von) to Wiener, November 29, 1946, JNLC, published in Rédei (ed.) 2005, 278–280.

162) Kay 2000a, 464. See also Kay 2000b.

163) Heims 1991, 94 ff.

the molecular mechanisms of living matter. Thus, Delbrück was suspicious of cybernetics, while von Neumann deemed it essential to involve him in the discussions. This made it easier to understand von Neumann's autonomous thinking within the cybernetics group: his outlook was focused on the development of an abstract theory of automata, which was considered promising also by geneticists. In this period he also kept up a correspondence with Alfred J. Lotka concerning the latter's work on population dynamics, as well as with the German physical chemist Karl Friedrich Bonhoeffer (with whom he was put in touch by Delbrück), concerning his studies on excitation and on the conduction of stimuli by means of a physical analogy.

In December 1949, in five lectures at the University of Illinois, von Neumann gave a second public presentation of his ideas on the theory and organization of complicated automata, this time before an audience of mathematicians and engineers. He also appears to have repeated this cycle of lectures for the staff of the Bell Telephone Laboratories.[164] He began with a comparative description of the structure and complexity of the computer and of the human nervous system, considered as the main examples of the abstract concept of "automaton". Leaving aside the "material" aspects of the components of both systems (the object of the study of physiology, in the case of the human brain, and of engineering science, in the case of computers), he moved on to an analysis of the organization of these components and of their interaction and group functioning, which was indeed the object of the theory of automata. The formal neuronal networks of McCulloch and Pitts represented the axiomatic model on which the theory rested. On these bases, von Neumann began to study the reliability, the complexity, and the self-reproduction of automata.

The question of *complexity* – or "complication", as von Neumann called it – of automata was approached by using the concept of the Turing machine, an abstract machine, capable of printing or deleting 0s and 1s on a tape of infinite length and thus of simulating any process that could be described by means of a finite number of logical operations. This concept was generalized by von Neumann in order to obtain a description of an abstract system of machines or automata capable of constructing other identical machines ("self-reproduction"), as well as of producing a more complex automaton than the original one. He later perfected this model, making use of the analogy with biological evolution, in which the complexity of the species increases, and including such aspects as increased efficiency in adapting to the environment and mutation.

After the Illinois lectures, he began writing a book that was to be published by the University of Illinois Press. However, his numerous commitments considerably slowed down completion of the final version; the text remained incomplete and was published almost ten years after his death. In 1966, his collaborator, Arthur Burks, edited the book *Theory of self-reproducing automata* including the Illinois lectures, together with another incomplete original work entitled "The theory of automata: Construction, reproduction, homogeneity", dealing with the so-called "cellular automata" and dating back to the years 1952–1953 (Neumann (von) 1966).

164) Aspray 1990, 320, note 90.

The first model of *self-reproduction* in automata presented several drawbacks, as they consisted of machines made up of a limited number of components situated in space, as it were, in a geometric environment: the system described was capable of subsuming these components and working with them. Von Neumann later abandoned this model, which was too closely linked to physico-geometrical considerations, and began to develop a more abstract model, partly at Ulam's instigation (Aspray 1990, 202). He was aware of the difficulties involved in following a criterion of formal simplicity in tackling a complex problem and, at the same time, in trying to remain close to real examples in order to avoid formulating a simply commonplace model. His second model, the cellular automaton, consisted of a two-dimensional array of square cells. Each cell allowed a finite number of states (29 in von Neumann's model), and the state of a cell at any given instant depended on the state of the neighbouring cells in the preceding instant: in this way it was possible to obtain certain cell groups that formed a system analogous to a living entity, capable of moving and of reproducing itself in the sense of being able to determine that another group of cells had entered a similar state.

The problem of the *reliability* of automata underlay a probabilistic theory of automata based on statistical information theory. This topic was developed in a series of lectures given by von Neumann at CalTech in January 1952 under the title of "Probabilistic logics and the synthesis of reliable organisms from unreliable components", published in 1956 by Shannon and John McCarthy in a collective book of studies on automata (Neumann (von) 1956). The underlying idea was to overcome the problems caused in the computer and in other automatic devices by the defective function of components that, being physical in nature, were subject to breakdown and to error. Already in his 1948 lecture at Pasadena, von Neumann had pointed out that also in living organisms dysfunctions may occur in various parts, and he compared the error control procedures in natural systems with those in artificial systems (Neumann (von) 1951). Natural systems do not need external intervention, because the organism is capable of diagnosing the error and minimizing its effects, and then of definitively blocking any defective components or, if possible, of repairing them in time. Conversely, artificial machines are designed in such a way that every error is amplified and can thus be identified more rapidly and allow the immediate repair or replacement of the defective component. «Our behavior is clearly that of overcaution, generated by ignorance», he wrote (Neumann (von) 1951, in JNCW, vol. 5, 306). The brain of animals and man is a complex and reliable system, the elements of which – neurons and their connections – are extremely fragile and unreliable: the brain continues to function even when parts of it are damaged. The problem presented was similar to the important problem in communication theory mentioned above, namely, coding for the transmission of information that is highly reliable, even when the transmission of signals and individual symbols is unreliable and subject to noise. Von Neumann actually devised procedures similar to those customarily found in communication theory.

Perhaps the best-known work by von Neumann in this field is his famous book, also published posthumously and in incomplete form, *The computer and the brain* (Neumann (von) 1958). In the introduction, he wrote that it consisted of «an ap-

proach toward an understanding of the nervous system from the mathematician's point of view». Essentially, this text summarizes the approach followed in the preceding works, although the logical and general theory of automata is situated in the background of the exposition and the emphasis is placed on the comparison between computer and brain. The first part is dedicated to a description of the fundamental principles of the computer and the second to a detailed exposition of the human nervous system as an information processing system. The comparison between the two systems took into account a large number of aspects, such as speed, energy consumption, size, and efficiency. This text, although only a draft, which von Neumann frantically wrote during the last few months of his life, reflects the brilliant clarity of his mind and represents a model of scientific literature. Furthermore, the theory of automata, and this book in particular, can be considered an intellectual testament left by von Neumann, the contribution which has had the strongest echo in the world of science and culture. The central idea contained in this text, which aroused surprise and produced so many favourable and unfavourable reactions, was reflected in rough and ready sensationalistic formulae such as the title of the article published by John Kemeny in the *Scientific American* magazine in 1955: "Man viewed as a Machine".

A general logical theory of automata in the meaning in which von Neumann conceived of it was not developed any further. His contribution as well as those of the cybernetic group were seminal works which formed the cultural background of 1950s and 1960s research in systems engineering and systems theory, regarding complex systems, decision processes, and the man-machine analogy. For example, in 1965 Lotfi A. Zadeh, a researcher from the University of California at Berkeley Electronics Research Laboratory published in the journal "Information and Control" his paper on fuzzy sets (Zadeh 1965) "as a natural way of dealing with problems in which the source of imprecision is the absence of sharply defined criteria of class membership rather than the presence of random variables (Ibidem, 339): this idea calls forth von Neumann's remarks on the limitation of classical logics and its "rigid, all-or-none concepts".[165]

Moreover, *The computer and the brain* is still read today by biomedical researchers, and its influence has radically changed the attitude towards the kind of comparison between life and artificial creations proposed in it. McCulloch and Pitts had run into serious difficulty in their attempt to publish their article, which had aroused such great interest in von Neumann: it was Nicholas Rashevsky who finally decided to publish it in his *Bulletin of Mathematical Biophysics*. Today, however, this type of approach enjoys much credence in the field of the cognitive sciences. Von Neumann's ideas essentially represent the direct precursors of "artificial intelligence", which is well known also to the general public. Nevertheless, contemporary "brain mechanics" often venture on to the dangerous ground of comparison or even

165) The influence of von Neumann on scientists and engineers working behind the "iron curtain" (the "enemy" in the Cold War) is difficult to assess. See for example Slava Gerovitch's work on cybernetics in the USSR (Gerovitch 2002), Anne Fitzpatrick et al. on the early years of development of the computer in the USSR (Fitzpatrick et al. 2006), and Alfred Zauberman's work on mathematical economics in the socialist countries (Zauberman 1969, 1975, 1976).

of identification between man and the machine; and it is interesting to recall the serious obstacles that stand in the way of such analogies and identification and of which, as Shannon (1958, 127–128) pointed out, von Neumann was perfectly well aware. And it was actually Shannon who remarked on the interesting comparison between the language of mathematics and the language of the brain with which von Neumann concludes *The computer and the brain*:

> *Pursuing this subject further gets us necessarily into questions of* language. *As pointed out, the nervous system is based on two types of communication: those which do not involve arithmetical formalisms, and those which do, i.e., communications of orders* (*logical ones*) *and communications of numbers* (*arithmetical ones*). *The former may be described as language proper, the latter as mathematics.*
>
> [. . .] *Just as languages like Greek or Sanskrit are historical facts and not absolute logical necessities, it is only reasonable to assume that logics and mathematics are similarly historical, accidental forms of expression. They may have essential variants, i.e., they may exist in other forms than the ones to which we are accustomed. Indeed, the nature of the central nervous system and of the message system that it transmits indicate positively that this is so. We have now accumulated sufficient evidence to see that whatever language the central nervous system is using, it is characterized by less logical and arithmetical depth than we are normally used to. The following is an obvious example of this: the retina of the human eye performs a considerable reorganization of the visual image as perceived by the eye. Now this reorganization is effected on the retina, or to be more precise, at the point of entry of the optic nerve by means of three successive synapses only, i.e., in terms of three consecutive logical steps. The statistical character of the message system used in the arithmetics of the central nervous system and its low precision also indicate that the degeneration of precision, described earlier, cannot proceed very far in the message systems involved. Consequently, there exist here different logical structures from the ones we are ordinarily used to in logics and mathematics. They are, as pointed out before, characterized by less logical and arithmetical depth than we are used to under otherwise similar circumstances. Thus logics and mathematics in the central nervous system, when viewed as languages, must structurally be essentially different from those languages to which our common experience refers.* (Neumann (von) 1958, 81)

Turing had observed that the process of "imitation" between computing machines could be based on the following procedure: a first computing machine transmits commands to a second one, translating them into an order sequence by means of a code ("short code") in such a way as to get it to work as it pleases. In this way, the first machine may appear to resemble the second one, even though it actually works with

a different language. Von Neumann had pointed out that if the brain is thought of as a computing machine, it may be imagined that the external language it uses in communication is different from the internal language used for calculation. In other words, mathematics may be a secondary language, the short code built on the primary language actually used by the central nervous system: thus, the external forms of our mathematics would not be relevant from the point of view of evaluating what the mathematical or logical language *truly* used by the central nervous system is.

In this way, although von Neumann reiterated his pan-logical and pan-mathematical world view he did not go beyond a prudent formal analogy between brain and computer, quite distant from any ontological claim as to what language the brain "really" uses.

In the closing years of his life, driven by a great intellectual curiosity, von Neumann ventured on to ground that had hitherto remained practically unexplored. There is no doubt that he was getting ready to follow an innovative and breakaway path, as when he introduced game theory in economics, and with his customary self-confidence. One year after his death, in the commemorative issue of the *Bulletin of the American Mathematical Society*, Kuhn and Tucker wrote:

> *Von Neumann's interest in "problems of organized complexity", so important in the social sciences, went hand in hand with his pioneering development of large-scale high-speed computers. There is a great challenge for other mathematicians to follow his lead in grappling with complex systems in many area of the sciences where mathematics has not yet penetrated deeply.* (Kuhn, Tucker 1958, 120)

The scientific community, following the path opened up by von Neumann, was venturing on to the ground of "complexity" – a topic on which rivers of ink had flowed by the end of the twentieth century.

Concluding Remarks: von Neumann and Twentieth Century Science

The twentieth century witnessed extraordinary changes in the structure and functions of science. The collapse of the ancient consolidated certainties was accompanied by an impressive flourishing of science and by the invasion of practically every branch of thought and human activity by scientific thinking and practice.

The "loss of certainty", sometimes traumatically, marked the early decades of the century. The emergence of Einstein's theory of relativity marked a crisis in the Newtonian conception of space, time and motion, while quantum theory challenged one of the strongholds of reductionism, that is, the representation of phenomena as "continuous" processes. Infinitesimal calculus was the mathematical tool *par excellence* used in this representation, and so also its centrality was swept away. But another hard blow was struck against classical science when quantum mechanics challenged its very essence – objectivism. The impossibility of eliminating the observer's role in the description of microscopic phenomena was a mortal blow to the principle of objectivity, which was precisely what made scientific thinking something different and "superior" (inasmuch as it was more "certain") than other forms of thinking. In the field of mathematics, the most serious wound was that of the crisis of the validity of foundations and then the failure of the Hilbertian formalist programme, which seemed to confirm the harsh necessity for mathematicians to coexist with the impossibility of proving the internal consistency of their discipline.

Einstein and Hilbert were the emblematic figures of the difficult early decades of the century. They may be considered more than others as "responsible" (or at least as the most characteristic representatives) of a process that shook classical science to its very foundations: Einstein introduced a new view of the relationship between physics and mathematics (which founded the basic premises of a modelling approach), Hilbert an abstract and axiomatic view of mathematics. Nevertheless, both of them were at the same time hostile to the idea that science should renounce the sacred principles of objectivity, truth and unity. Both the unified theory of gravitation and the formalist programme towards a self-consistent refoundation of mathematics were ex-

pressions of firm determination – after the storm had passed – to restore to science new, consistent and unified foundations, to define the tenets of a new reductionism. In other words, Einstein and Hilbert (like many other scientists of the time) rejected the prospect of a science that was inexorably broken up into a thousand rivulets of incoherent knowledge that could not be related back to a well-defined nucleus of principles; and that it was necessary to renounce the idea that physics offers a “true” image of reality and that mathematics offers results which are “certain” on the plane of logic.

And yet, despite this crisis at the epistemological level, the scientific conception of the world gradually invaded all sectors of knowledge, in particular the human and social sciences. Technology also underwent a transformation into a form of theoretical knowledge imbued with scientific concepts and procedures and leaving increasingly less room for traditional ideas and methods: from *techne* and art, a definitive transition was made to technology. It is sufficient to compare nineteenth century electrical or chemical engineering with the theoretical, mathematical electronic engineering of the early twentieth century. But also medicine, military strategy or management lost what previously characterized them as arts that could not be reduced to the scientific method; or at least, they were bent to conform to the scientific image, almost universally considered as a guarantee of the rationality of *praxis*.

This paradoxical triumph of a fragile science was not without its consequences to the direction of twentieth century scientific research. Also science was transformed by its embrace with technology and the “sciences of the artificial” – to use the term coined by Herbert Simon in 1967. Reference is made to the hybrid born as a result using the word “technoscience”. Technoscience increasingly adopted as its criterion of development, not “truth” – which had been found to be an unattainable ambition – but rather “effectiveness” and “utility”, that is, the criteria typical of technical knowledge. The basic motive was less and less that of “explaining” but more and more that of “describing” and “controlling” phenomena; the question asked was more and more rarely “is it true?” and more and more frequently “does it work?”' or “is it useful?” Mathematics was no longer viewed as “the” language in which nature was written and became a set of useful and effective representations of phenomena lacking all characteristics of unity, that is – in a word – a set of “models”. On top of it all, this proved to be a winning point of view: it opened up vast horizons for the development of technology, and also mathematics benefited, thanks to the growth of applied mathematics.

However, it would be erroneous to believe that the majority of scientists had abandoned the more or less explicitly cultivated idea that science is “different” from other forms of human activity, insofar as it pursues objective and certain knowledge, or at least knowledge that is more objective and more certain than other forms of knowledge. A strong tension thus sprang up between this ancient and consolidated need and the new trends described above: this tension was to spread throughout the whole of twentieth century science and continues to be unresolved.

Einstein and Hilbert, although the principal initiators of the new process, were still men of the nineteenth century judging by their unconditional attachment to the primacy of the ideal of truth and unity of science. Von Neumann is a different case: he was not only an initiator of the new process, but one of its main actors, as far as

its extreme consequences. And yet also von Neumann was linked (no less than his teachers) to ideals of scientific reductionism, although he practised with determination a science capable of spreading in a pragmatically effective way to all branches of human activity. This is why the tension mentioned above is even more evident and illuminating as far as his figure is concerned. Von Neumann's efforts to embrace every aspect of science, technology and praxis in a unitary view represents an extraordinary intellectual challenge that expresses – regardless of its successes and failures – all the benefits and risks of the new phase that opened up during the twentieth century, which broke the links with the view of Galileo and Newton.

The extremes of the span of von Neumann's scientific life are already an effective representation of that tension and of the tremendous effort he made to resolve it. He began his career by investigating the abstract problems related to the foundations of mathematics and he concluded it on extremely concrete themes, ranging from problems regarding weapons and military strategy, to electronic calculators, mathematical programming and project management. And yet we have seen that also in these "utilitarian" applications – in which a modelling approach was used that was extremely distant from the classical mathematical descriptions of reality – he was guided by the historically remote problems with which he began his scientific career.

During the years of the diaspora of the Vienna Circle, he transferred the central ideas of this philosophical-scientific project from a devastated Europe to the United States: the primacy of a scientific conception of the world, the role of logical analysis, methodological materialism, the ideal of the unity of science. And this was not all. He was able to give these philosophical premises a concrete content thanks to the conviction he had developed after the crisis of the old Hilbertian formalist program, now defeated and obsolete – namely, the central role of the *axiomatic approach*. In other words, thanks to the cornerstone of von Neumann's work in order to exorcise the "monsters" haunting science, in particular set theory and quantum mechanics. Furthermore, it was the axiomatic approach that would later lead to further significant successes being obtained and to his fame as a universal scientist being consolidated, from game theory to computer architecture. Again, the theory of automata, his last great theoretical project, had as its leitmotiv the same attempt to reduce several outstanding postwar developments – from control engineering to neurophysiology, from molecular genetics to computer science – to a logical-axiomatic formulation of several general principles and problems.

The *panmathematical* and *axiomatic* approach was the ground on which von Neumann endeavoured, in a supreme intellectual effort that consistently runs through all his work and activities, to reconstruct a *unity* among the thousand separate trickles into which scientific research was being reduced. It would have been an illusion to try to relate these trickles back to a single flow of classical science, based on the unitary cement of a few universal laws – something that he was well aware of. Thus, the unifying element was identified by him as a methodical conceptual approach: axiomatic, abstract and logico-mathematical.

The distance between *von Neumann's reductionism* (and it must be called reductionism) and classical reductionism is easily measured by the way he viewed the

brain-computer analogy in his theory of automata. He is rightly considered one of the founding fathers of modern information science and of the "artificial intelligence" program based on the brain-computer analogy. And yet, although this analogy is considered (as is frequently the case nowadays) as an attempt to provide a "scientific" demonstration that the human brain *is* a computer, this can hardly be blamed on von Neumann. What he did was to construct an axiomatic model of a complex of properties related to the functioning of the nervous system which formed a basis for the imitation of the human brain – or, to be more precise, of certain of its aspects – and to build the computers. Conversely, the abstract logical computer schema became a mathematical logical model suitable for representing a series of aspects related to the functioning of the human brain. Nothing more: this model could not be used to demonstrate the metaphysical assumption that the human brain *is really* a computer. Indeed, von Neumann himself clearly underlined the break with the classical paradigm and with all ontological wishful thinking when he pointed out that science "does not try to explain" "it hardly even tries to interpret", above all "it makes models". Current interpretations of the brain-computer analogy viewed as a demonstration that the brain *is* a computer thus merely prove that the ambitions of metaphysical claims are much stronger than a certain prepackaged positivism would have us believe.

Even though von Neumann was convinced of the need to give up all attempts to use metaphysical principles as a unifying basis, he never renounced the idea of the unity of science. His neo-reductionist proposal was to choose axiomatic language and formal logic as the common ground for unification. His approach left aside all value judgments and pointed the way towards a coherent solution to the problem of the relationship between scientific analysis and real phenomena, between theory and empirical verification, without taking any metaphysical or empiricistic shortcuts disguised under false scientific pretences. Even though his pan-mathematical and pan-logical conception of the world and his latent penchant for a deterministic view revealed the persistent influence of the metaphysical ambitions mentioned above.

The figure of von Neumann illuminates two other aspects of twentieth century science, that is the relationship between science and technology and the problems raised by the use of science in the industrial world, in military activities and – in a broad sense – at the service of "power". We have described von Neumann's leading role in the new forms of interrelationship between science, technology, society and politics.

In this connection, it is quite wrong and unproductive to view this interrelationship in an abstract and metahistorical way, without referring to the historical trajectory of the twentieth century and the lives of the people who lived through it. In our view, this approach has hindered the understanding of the figure of von Neumann, distorting and diminishing his image, and reducing it practically to the caricature of a scientist subservient to all the needs of power, even the least noble. Indeed, the case of von Neumann has been used to support the thesis of a twentieth century science completely 'at the service' of technologies of use to power and war. As a consequence, in this view, not only science was perverted from its "original" goals, but power also

conditioned the scientific conception of the phenomena: thus, the key conceptions of twentieth century technoscience, such as information, control and network, derive directly from the wartime context. We consider however that we have illustrated on the contrary how these ideas, which were a byproduct of technical thought, have entered scientific thought precisely through a mingling of science and technology to which a strong impulse was given on several occasions by wartime needs – or should we say defense needs – of modern societies, on the basis of the leitmotiv that science must play an essential role in human progress.

Our aim in approaching such a rich and complex figure – on whose work the historiographical scrutiny is still far from complete – was indeed to make contact with the historical upheavals of the past century. Like other European scientists, von Neumann witnessed the end of the political and cultural supremacy of the Old Continent caused by the dreadful events of the two world wars, the advent of nazism in Germany and the communist revolution in Russia. Then, more than the others, he played a leading role in the rise of the United States to its new status of political and cultural world leader that it has retained down to our times, so unchallenged as to have led to the revival of the word "empire". This rise took place in specific historical circumstances, those of the Cold War. Starting in the late 1960s and 1970s, the protest and counter-culture movements in the United States and its political allies gave rise to a harsh criticism of the western capitalist world that challenged and demonized technoscience on the basis of ideological theories and its social role. Little importance was attached at the time to the fact that the choices proposed by von Neumann have always been implemented on the basis of consolidated and convinced personal ideas, therefore not unlike those of other scientists who influenced communist countries or those of scientists living in capitalistic, democratic countries and supporting opposite views to von Neumann's.

An historical and non-abstract perspective instead leads us to acknowledge in the United States during the Cold War – and before this, in the Great Britain of the resistance to nazism – a revival of ideas regarding the social role of science which had emerged for the first time in late seventeenth century France. Also at that time, during the final phase of the monarchy and above all during the revolutionary and Napoleonic period, scientists were recruited by the State and many subscribed to this appeal, viewing their choice as being dictated by the intention of introducing criteria of scientific rationality into productive and industrial decision-making, into the conduct of public life and into wartime affairs. In recent years, a rich historiography on the specific results has grown up (the reorganization of public finances, the "rational" conduct of factories, the creation of modern technical education) as well as on the accompanying philosophical outlook. Indeed, the Enlightenment assertion of science as a guarantee and almost as the exclusive equivalent of rationality grew by leaps and bounds in the 1940s and 1950s – all over the world, in the USA as in the USSR. These were the leaps and bounds of modernity, with its potential benefits and risks.

We pointed out in the Introduction that von Neumann represents the revival of the old Enlightenment ambition of setting science in a central position in society's decision-making process. Clearly his guiding principle was the rational determina-

tion of the conditions of lesser damage in very difficult conditions, also under the influence of a disenchanted view of human affairs held by a man who had witnessed an interminable sequence of tragedies and, in order to escape which, had renounced more than one homeland. Therefore, it is this ambition which must be examined in the light of the society it aims to construct, the society for which wars are actually fought, both yesterday and today. The key questions are: what are the limits of the role to be assigned to the scientific elite in a democratic society? Or else: can technoscience set its own limits of development and what interaction takes place between scientific projects, technical design, ethics, religious convictions? And again, can a monopoly on rationality be assigned to mathematics and technoscience, also as far as the governance of the economy and political life is concerned? On all these questions von Neumann's life and work provide elements of reflection, even where realities that were still unimaginable in his lifetime – such as the development of biotechnologies – are at stake. The figure of John von Neumann represents better than any other all the aspects of twentieth century science: its strengths and its weaknesses, the triumphs that have changed our everyday lives and the enormous unsolved problems still facing it.

Chronology

1903 János Lájos Neumann is born in Budapest
(he was later to use a German version of the name, Johann Ludwig von Neumann and finally an English version, John L. von Neumann: the "von" indicates a noble title acquired by his father in 1917).

1911 Begins his studies at the Lutheran Gymnasium of Budapest.

1919 First departure from Hungary, with his family, during the months of Bela Kun's Hungarian Soviet Republic.

1921 Enrolls at the University of Budapest and begins his studies in Berlin.

1922 Publication, in collaboration with his private tutor M. Fekete, of his first article on mathematics in the mathematical journal *Jahresbericht der Deutschen Mathematiker Vereinigung*.

1923 Begins chemical engineering course at the Zurich Polytechnic.

1925 Obtains engineering diploma (*Diplomingenieur*).
Ph.D. in mathematics at the University of Budapest, on the axiomatization of set theory.

1926 Rockefeller Foundation fellowship for advanced studies at Göttingen.
Collaboration with Hilbert and Nordheim in research on the axiomatic foundation of quantum mechanics.

1927 *Privatdozent* in Berlin.

1928 Publication of first paper on the theory of games.

1929 *Privatdozent* at Hamburg.

1930 Married Marietta Kövesi.
First stay at the Princeton University as visiting lecturer in mathematical physics during the Spring term.

1932 Publication of his first book *Mathematische Grundlagen der Quantenmechanik*.

1933 Appointed professor at the Institute for Advanced Study at Princeton.

1935 Birth of daughter Marina.

1937 Divorce from Marietta Kövesi.
Obtains US citizenship.

Begins collaboration with Army Ordnance Department Ballistic Research Laboratory, located at Aberdeen (Maryland).
Holds the American Mathematical Society XXth Colloquium Lectures on "Continuous Geometry".

1938 Married Klára Dán.
Awarded Bôcher Prize of the American Mathematical Society.

1941 Nominated advisor of the Navy Bureau of Ordnance, Washington, D.C.

1943 Begins collaboration with the Los Alamos Scientific Laboratory (New Mexico), which was to last until 1955.
Trip to the UK for war-related scientific activities.

1944 Publication of the book *Theory of games and economic behavior*, in collaboration with O. Morgenstern.
Holds the American Mathematical Society "Gibbs Lectures" on "The ergodic theorem and statistical mechanics".

1947 Medal for Merit awarded by the President of the United States and Distinguished Civilian Service Award of the US Navy.

1948 Advisor of the RAND Corporation.

1950 Publication of the two-volume book *Functional operators*.

1951 Appointed president of the American Mathematical Society, a post held until 1953.
Appointed member of the Air Force Scientific Advisory Board.
Begins his activity of industrial consultancy under contract with IBM and other private corporations

1952 Appointed member of the Atomic Energy Commission General Advisory Committee
Advisor of the Central Intelligence Agency.

1953 Appointed member of the National Security Agency Advisory Board.
Appointed member of the Air Force Strategic Missiles Evaluation Committee, renamed in 1954 ICBM Scientific Advisory Committee (chaired by von Neumann)

1955 Appointed member of the Atomic Energy Commission.

1956 Atomic Energy Commission Enrico Fermi Science Award
Medal of Freedom awarded by the President of the United States
Died in Washington, D.C.
Posthumous publication of the Yale University Silliman Lectures under the title *The computer and the brain*.

Bibliography

Sources and scholarship on John von Neumann

In May 1958, one year after von Neumann's death, a monographic issue of the *Bulletin of the American Mathematical Society* was dedicated to his life and work. A short biographical outline by Stanislaw Ulam was followed by an analysis of the main fields of research investigated by von Neumann, written by colleagues and collaborators, among them Garrett Birkhoff, Paul Halmos, Harold Kuhn, Claude Shannon, and Albert Tucker. Between 1957 and 1959 also the Hungarian mathematical journal *Mathematikai és Physikai Lapok* published several analyses of his work.

In the 1980s, several meetings to commemorate von Neumann were held: in Bucharest, in 1987, thirty years after his death; in the same year a Congress was held on "The computer and the brain" at Arizona State University; in 1988 the AMS organized a Symposium at Hofstra University on "Von Neumann's Legacy" (Glimm, Impagliazzo, and Singer 1990). Again meetings in Hungary (at the Hungarian Academy of Science in Budapest, during the Centennial Conference on Linear Operators and Foundation of Quantum Mechanics) and the United States (during the SIAM Conference on Computational Science and Engineering in San Diego) were held in 2003 to commemorate the birthday centennial (Davis 2003).

Ulam's papers, conserved in the library of the American Philosophical Society, include a draft biography of von Neumann that was never published (Stanislaw M. Ulam Papers, series VIII, box 1). Von Neumann's brother, Nicholas A. Vonneumann, is the author of a biography based essentially on personal recollections (Vonneumann 1987). A biographical and scientific profile has been drawn in the form of parallel lives by Steve J. Heims in his book *John von Neumann and Norbert Wiener. From mathematics to the technologies of life and death* (Heims 1980). William Aspray's *John von Neumann and the origins of modern computing* (Aspray 1990) – a book that appeared in the months between the fall of the Berlin wall and the end of the Soviet Union – included in Chapters 1 and 9 a biographical reconstruction based on archival material and oral interviews.

Indeed, despite the numerous partial studies on von Neumann's scientific work (references will be found in the general bibliography below), many aspects of his manifold activities are still relatively unknown. His image, as it emerges from the

studies published hitherto is, on the one hand, that retained in the memory of the community of researchers and, on the other, a largely fragmented memory: there is not one but several different von Neumanns, and even those who have a thorough knowledge of his scientific work or his activity often know nothing about the rest. Furthermore, there is a schizophrenic contrast between the laudatory and reverential tone of prefaces and commemorations and books and papers that reduce von Neumann's figure to a Cold War hawk. His intellectual trajectory is closely interwoven with the circumstances of his life and with the historic events forming the background to his personal life and his scientific activity. Our goal has been to offer a cultural reconstruction of his work, life and thought in the context of his epoch, drawing also on recent research on the history of science and technology in the first half of the twentieth century and the Cold War – the reader will find references to this literature in the general bibliography presented after the list of von Neumann's published works mentioned in the book.

John von Neumann's published works and archived papers

In years 1961–63, six volumes of his *Collected works* were published, edited by Abraham Taub, including hitherto unpublished material deposited by von Neumann in the IAS library at Princeton, the re-ordering and cataloguing of which was funded by the ONR (JNCW, see the complete bibliographical reference below). The JNCW comprise articles, extracts from books, reports to government agencies – some of which were still secret when the book was being prepared – and selected unpublished material. Other previously unpublished papers have been published in the book *Papers of John von Neumann on computing and computing theory* edited by William Aspray and Arthur Burks (Aspray, Burks 1987). A collection of selected works is available under the title *The Neumann compendium* (Bródy, Valmos 1995).

Von Neumann's papers are today conserved mainly in the Manuscript Division of the Library of Congress in Washington (John Von Neumann Collection, 1913–1992, see below the reference of the Catalogue). Other archival material is deposited in the Library of the Hungarian Academy of Sciences in Budapest (Department of Manuscripts and Rare Books), in the Archives of American Mathematics (Center for American History, The University of Texas at Austin) and in the IAS Archives located in the IAS Historical Studies-Social Science Library. A selection of letters conserved in the Washington and other archives has been published by Miklós Rédei (Rédei (ed.) 2005).

JNLC is the abbreviated reference for the archival collection John von Neumann Papers, Manuscript Division, Library of Congress, Washington D.C. The catalogue is an electronic resource of the Library of Congress:

JOHN VON NEUMANN. *A Register of His Papers in the Library of Congress*, prepared by Margit Kerwin and Patrick Kerwin, revised and expanded by Patrick Kerwin, Washington, D.C.: Manuscript Division, Library of Congress, 2002.

JNCW is the abbreviated reference for the 6-volumes of John von Neumann's *Collected works*:

NEUMANN, J. (VON) 1961–1963. *Collected works*, edited by A.H. Taub. New York: Macmillan, 6 volumes.

Works by von Neumann mentioned in the book

Works written by von Neumann alone or in collaboration are listed in chronological order. The references in square brackets are to the *Collected works*. The *Collected works* include research contracts reports (such as Neumann (von) 1945a and 1949c) not easily available otherwise, von Neumann's statement before the Special Senate Committee on Atomic Energy (Neumann (von) 1946), as well the following three unpublished works mentioned by us: Neumann (von), Goldstine 1946; Neumann (von) 1947b, edited by A.W. Tucker and H.W. Kuhn; and Neumann (von) 1948. Fekete, Neumann 1922 as well as Neumann 1923, 1925a, and 1926 were published under the name János Neumann. A full list of von Neumann's works can be found in Aspray 1990, 357–367.

FEKETE, M., and J. NEUMANN 1922, "Über die Lage der Nullstellen gewisser Minimumpolynome," *Jahresbericht der Deutschen Mathematiker-Vereinigung* 31: 125–130 [Vol. I, no. 2].

NEUMANN, J. 1923, "Zur Einführung der transfiniten Zahlen," *Acta Scientiarum Mathematicarum Szeged* 1: 199–208 [Vol. I, no. 3].

NEUMANN, J. 1925, "Egeyenletesen sürü számsorozatok," *Mathematikai és Physikai Lapok* 32: 32–40 [Vol. I, no. 5].

NEUMANN (VON), J. 1925, "Eine Axiomatisierung der Mengenlehre," *Journal für Mathematik* 154: 219–240 [Vol. I, no. 4].

NEUMANN, J. 1926, "Zur Prüferschen Theorie der idealen Zahlen," *Acta Scientiarum Mathematicarum Szeged* 2: 193–227 [Vol. I, no. 6].

NEUMANN (VON), J. 1927, "Zur Hilbertschen Beweistheorie," *Mathematische Zeitschrift* 26: 1–46 [Vol. I, no. 12].

HILBERT, D., J. VON NEUMANN, and L. NORDHEIM 1927, "Über die Grundlagen der Quantenmechanik," *Mathematische Annalen* 98: 1–30 [Vol. I, no. 7].

NEUMANN (VON), J. 1928a, "Die Axiomatisierung der Mengenlehre," *Mathematische Zeitschrift* 27: 669–752 [Vol. I, no 16].

NEUMANN (VON), J. 1928b, "Zur Theorie der Gesellschaftsspiele," *Mathematische Annalen* 100: 295–320 [Vol. VI, no.1] (English translation in Tucker, A.W., and

D. Luce, eds. 1959, *Contributions in the Theory of Games*, vol. IV, Princeton, N.J., Princeton University Press: 13–42).

NEUMANN (VON), J., and E. WIGNER 1928a, "Zur Erklärung einiger Eigenschaften der Spektren aus der Quantenmechanik des Dreheelektrons, I," *Zeitschrift für Physik* 48: 868–881 [Vol. I, no. 18].

NEUMANN (VON), J., and E. WIGNER 1928b, "Zur Erklärung einiger Eigenschaften der Spektren aus der Quantenmechanik des Dreheelektrons, II," *Zeitschrift für Physik* 49: 73–94 [Vol. I, no. 19].

NEUMANN (VON), J., and E. WIGNER 1928c, "Zur Erklärung einiger Eigenschaften der Spektren aus der Quantenmechanik des Dreheelektrons, III," *Zeitschrift für Physik* 51: 844–858 [Vol. I, no. 20].

NEUMANN (VON), J. 1929a, "Beweis des Ergodensatzes und des H-Theorems in der neuen Mechanik," *Zeitschrift für Physik* 57: 30–70 [Vol. I, no. 25].

NEUMANN (VON), J. 1929b, "Allgemeine Eigenwerttheorie Hermitescher Funktionaloperatoren," *Mathematische Annalen* 102: 49:131 [Vol. II, no. 1].

NEUMANN (VON), J. 1929c, "Zur Algebra der Funktionaloperatoren und Theorie der normalen Operatoren," *Mathematische Annalen* 102: 370–427 [Vol. II, no. 2].

NEUMANN (VON), J., and E. WIGNER 1929a, "Über merkwürdige diskrete Eigenwerte," *Physikalische Zeitschrift* 30: 465–467 [Vol. I, no. 23].

NEUMANN (VON), J., and E. WIGNER 1929b, "Über das Verhalten von Eigenwerten bei adiabatischen Prozessen," *Physikalische Zeitschrift* 30: 467–470 [Vol. I, no. 24].

NEUMANN (VON), J. 1931, "Die formalistische Grundlegung der Mathematik," *Erkenntnis* 2: 116–121 [Vol. II, no. 9].

NEUMANN (VON), J. 1932a, *Mathematische Grundlagen der Quantenmechanik*. Berlin, Springer. Other editions: New York, Dover, 1943; French translation, Paris, Presses Universitaires de France, 1947; Spanish translation, Madrid, Instituto de Matemáticas "Jorge Juan," 1949; English translation by R.T. Beyer, Princeton, N.J., Princeton University Press, 1955; Italian translation by G. Boniolo, Padova, Il Poligrafo, 1998.

NEUMANN (VON), J. 1932b, "Proof of the quasi-ergodic hypothesis," *National Academy of Sciences Proceedings* 18: 70–82 [Vol. II, no. 12].

NEUMANN (VON), J. 1932c, "Physical applications of the ergodic hypothesis," *National Academy of Sciences Proceedings* 18: 263–266 [Vol. II, no. 13].

NEUMANN (VON), J. 1933, "Die Einführung analytischer Parameter in topologischen Gruppen," *Annals of Mathematics* 34: 170–190 [Vol. II, no. 19].

NEUMANN (VON), J. 1934, "Almost periodic functions in a group I," *Transactions of the American Mathematical Society* 36: 445–492 [Vol. II, no. 23].

NEUMANN (VON), J., and S. BOCHNER 1935, "Almost periodic functions in a group II," *Transactions of the American Mathematical Society* 37: 21–50 [Vol. II, no. 26].

NEUMANN (VON), J. 1936, "The uniqueness of Haar's measure," *Matematiceskij Sbornik* 1: 721–734 [Vol. IV, no. 6].

NEUMANN (VON), J., and G. BIRKHOFF 1936, "The logic of quantum mechanics," *Annals of Mathematics* 37: 823–843 [Vol. IV, no. 7].

NEUMANN (VON), J., and F.J. MURRAY 1936, "On rings of operators," *Annals of Mathematics* 37: 116–229 [Vol. III, no. 2].

NEUMANN (VON), J. 1937, "Über ein ökonomisches Gleichungssystem und eine Verallgemeinerung des Brouwerschen Fixpunktsatzes," *Ergebnisse eines Mathematischen Kolloquium* 8: 73–83 (English translation Neumann (von) 1945b).

NEUMANN (VON), J., and F. J. MURRAY 1937, "On rings of operators, II," *Transactions of the American Mathematical Society* 41: 208–248 [Vol. III, no. 3].

NEUMANN (VON), J. 1940, "On rings of operators, III," *Annals of Mathematics* 41: 94–161 [Vol. III, no. 4].

NEUMANN (VON), J., and R. H. KENT 1940, *The estimation of the probable error from successive differences*, Ballistic Research Laboratory Report No. 175, Aberdeen, February 14, 19 pp.

NEUMANN (VON), J., R. H. KENT, H.R. BELLINSON, and B. I. HART 1941, "The mean square successive differences," *Annals of Mathematical Statistics* 12:153–162 [Vol. IV, no. 33].

NEUMANN (VON), J., and F. J. MURRAY 1943, "On rings of operators, IV," *Annals of Mathematics* 44: 716–808 [Vol. III, no. 5].

NEUMANN (VON), J., and O. MORGENSTERN 1944, *Theory of games and economic behaviour*, Princeton, N.J.: Princeton University Press (2nd ed. 1947, 3rd ed. 1953).

NEUMANN (VON), J. 1945a, *First draft on a report on the EDVAC*, Report prepared for US Army Ordnance Department and the University of Pennsylvania under Contract W-670-ORD-4926, June 30. Reprinted in Stern 1981: 177–246.

NEUMANN (VON), J. 1945b, "A model of general economic equilibrium," *Review of Economic Studies* 13: 1–9 [Vol. VI, no. 3].

NEUMANN (VON), J. 1946a, "Statement of John von Neumann before the Special Senate Committee on Atomic Energy, 31 January," Hearings before the Special Committee on Atomic Energy, U.S. Senate, 77th Congress, 2nd session, on S. 1717. A Bill for the Development and Control of Atomic Energy [Vol. VI, no. 37].

NEUMANN (VON), J., and H. H. GOLDSTINE 1946, "On the principles of large scale computing machines," in JNCW, vol. V: 1–32.

NEUMANN (VON), J., A. W. BURKS, and H. H. GOLDSTINE 1946, *Preliminary discussion of the logical design of an electronical computing instrument*, Part I, Vol. I, Report prepared for US Army Ordnance Department under contract W-36–034-ORD-7481 [Vol. V, no. 2].

NEUMANN (VON), J. 1947a, "The Mathematician," in: *The Works of the Mind*, R.B. Heywood (ed.), Chicago, University of Chicago Press: 180–196 [Vol. I, no. 1].

NEUMANN (VON), J. 1947b, "Discussion of a maximum problem," in JNCW, vol. VI: 89–95.

GOLDSTINE, H., and J. VON NEUMANN 1947, "Numerical inverting of matrices of high order," *Bulletin of the American Mathematical Society* 53: 1021–1099 [Vol. V, no. 14].

GOLDSTINE, H., and J. VON NEUMANN 1947–1948, *Planning and coding problems for an electronic computing instrument*. Part II, Vol. I–III, Report prepared for US Army Ordnance Department under contract W-36–034-ORD-7481 [Vol. V, no. 3, 4, 5].

NEUMANN (VON), J. 1948, *First report on the numerical calculation of flow problems*, 22 June–6 July, in JNCW, vol. V: 664–712.

NEUMANN (VON), J. 1949a, "Review of *Cybernetics* by N. Wiener," *Physics Today* 2: 33–34.

NEUMANN (VON), J. 1949b, "On rings of operators. Reduction theory," *Annals of Mathematics* 50: 401–485 [Vol. III, no. 7].

NEUMANN (VON), J. 1949c, *Recent theories of turbulence*, Report made to the ONR, in JNVW, vol. VI: 437–472.

NEUMANN (VON) J. 1950, *Functional operators*, Princeton, N.J., Princeton University Press, 2 vols.

NEUMANN (VON) J. 1951, "The general and logical theory of automata," in *Cerebral mechanisms in behavior – The Hixon Symposium, 1948*, L.A. Jeffrees (ed.), New York, Wiley: 1–31. [Vol. V, no. 9]

GOLDSTINE, H., and J. VON NEUMANN 1951, "Numerical inverting of matrices of high order. II," *Proceedings of the American Mathematical Society* 2: 188–202 [Vol. V, no. 15].

NEUMANN (VON), J. 1953a, "Communication on the Borel notes," *Econometrica* 21: 124–125 [Vol. VI, no. 2].

NEUMANN (VON), J. 1953b, "A certain zero-sum two-person games equivalent to the optimal assignment problem," in *Contributions to the theory of games*, vol. II, H.W. Kuhn, and A.W. Tucker (eds.), Princeton, N. J., Princeton University Press: 5–12 [Vol. VI, no. 5].

NEUMANN (VON), J. 1954a, "The Role of Mathematics in the Sciences and in Society," Address at 4th Conference of Association of Princeton Graduate Alumni, June 1954: 16–29 [Vol. VI, no. 34].

NEUMANN (VON), J. 1954b, "A numerical method to determine optimum strategy," *Naval Research Logistics Quarterly* 1: 109–115 [Vol.VI, no. 7].

NEUMANN (VON), J. 1955a, "Method in the Physical Sciences," in *The Unity of Knowledge*, L. Leary (ed.), New York, Doubleday: 157–164 [Vol. VI, no. 35].

NEUMANN (VON), J. 1955b, "Can we survive technology?," *Fortune*, June [Vol. VI, no. 36].

NEUMANN (VON), J. 1955c, "Impact of Atomic Energy on the Physical and Chemical Sciences" (Speech at MIT Alumni Day Symposium), *Technology Review* November: 15–17 [Vol. VI, no. 37].

NEUMANN (VON), J. 1955d, "Defense in Atomic War," in *Scientific Bases of Weapons*, *Journal of American Ordnance Association*, Washington, D.C.: 21–23 [Vol. VI, no. 38].

NEUMANN (VON), J. 1956, "Probabilistic logics and the synthesis of reliable organisms from unreliable components," in *Automata studies*, C.E. Shannon, and J. McCarthy (eds.), Princeton, N.J., Princeton University Press: 43–98 [vol. V, 329–378].

NEUMANN (VON), J. 1958, *The computer and the brain*, New Haven, CT, Yale University Press.

NEUMANN (VON), J. 1960, *Continuous geometry*, Princeton, N.J., Princeton University Press.

NEUMANN (VON), J. 1996, *Theory of self-reproducing automata*, A. Burks (ed.), Urbana, Ill., University of Illinois Press.

NEUMANN (VON), J. 1999, *Invariant measures*, Providence, R.I., American Mathematical Society.

General bibliography

Actes du Congrès International de Philosophie Scientifique, Sorbonne, Paris, 1935. Paris, Hermann, 8 vol.

AHRENS, W. 1900, "Mathematische Spiele," in *Encyklopädie der Mathematischen Wissenschaften mit Einschluss ihrer Anwendungen*, vol. I, W.F. Meyer (ed.), Leipzig, Teubner: 1080–1093.

ALDER, K. 1999, *Engineering the revolution: Arms and Enlightenment in France: 1763–1815*, Princeton, N.J., Princeton University Press.

ARCHIBALD, R.C., ed. 1938, *A semicentennial history of the American Mathematical Society 1888–1938*, Semicentennial Publications, No. 1, Providence, R.I., American Mathematical Society.

ARROW, K.J. 1951, *Social choice and individual values*, New York, Wiley.

ARROW, K.J. 1989, "Von Neumann and the existence theorem for general equilibrium," in Dore, Chakravarty, and Goodwin, eds. 1989: 15–28.

ARROW K.J., and G. DEBREU 1954, "Existence of an equilibrium for a competitive economy," *Econometrica* 22: 265–290

ASPRAY, W. 1988, "The emergence of Princeton as a world center for mathematical research, 1896–1939," in Aspray, and Kitcher, eds. 1988: 346–366.

ASPRAY, W. 1989, "The transformation of numerical analysis by the computer: An example from the work of John von Neumann," in Rowe, and McCleary, eds. 1989: 307–322.

ASPRAY, W. 1990, *John von Neumann and the origins of modern computing*, Cambridge, MA, MIT Press.

ASPRAY W., and A. W. BURKS 1987, *Papers of John von Neumann on computing and computer science*, Cambridge, MA, MIT Press/Los Angeles, Tomash Publishers.

ASPRAY, W., and PH. KITCHER, eds. 1988, *History and philosophy of modern mathematics*, Minneapolis, MN, University of Minnesota Press.

AUBIN, D. 1997, "The withering immortality of Nicolas Bourbaki: A cultural connector at the confluence of mathematics," *Science in Context* 10: 297–342.

AUMANN, R. 1985, "What is game theory trying to accomplish?," in *Frontiers in economics*, K. Arrow, and S. Honkapohja (eds.), Oxford, Basil Blackwell: 28–76.

AUSEJO, E., and M. HORMIGÓN, eds. 1993, *Messengers of mathematics: European mathematical journals* (1800–1946), Madrid, Siglo XXI.

AYER A.J., ed. 1959, *Logical positivism*, Glencoe, Ill., The Free Press.

BAKER, K.M. 1975, *Condorcet. From natural philosophy to social mathematics*, Chicago, The University of Chicago Press.

BEAULIEU, L. 1990, *Bourbaki. Une histoire du groupe de mathématiciens français et de ses travaux* (1934–1944), Ph. D. Thesis, Université de Montréal, 2 vols., 552 pp.

BENNETT, S. 1979–1993, *A history of control engineering*, vol. 1: 1800–1930, vol 2: 1930–1950, London, Peter Peregrinus.

BENNETT S. 2004, "Technological concepts and mathematical models in the evolution of control engineering," in Lucertini, Millán Gasca, Nicolò, eds. 2004: 103–128.

BERNKOPF, M. 1966–67, "The development of function spaces with particular reference to their origins in integral equation theory," *Archives for the History of Exact Sciences* 3: 1–96.

BIGELOW, J. 1980, "Computer development at the Institute for Advanced Study," in Metropolis, Howlett, and Rota, eds. 1980: 291–310.

BIRKHOFF, G. 1958, "Von Neumann and lattice theory," *Bulletin of the American Mathematical Society* 64: 50–56.

BIRKOFF, G. 1977, "Some leaders in American mathematics: 1891–1941," in Tarwater, ed. 1977: 25–78.

BOCHNER, S. 1958, *John von Neumann 1903–1957. A biographical memoir*, Washington, D.C., National Academy of Sciences.

BOREL, É. 1921, "La théorie du jeu et les équations intégrales à noyau symétrique," *Comptes Rendus de l'Académie des Sciences de Paris* 173: 1304–1308.

BOREL, É. 1924, "Sur les jeux où le hasard se combine avec l'habileté des joueurs," *Comptes Rendus de l'Académie des Sciences de Paris* 178: 1304–1308.

BOREL, É. 1927, "Sur les systèmes de formes linéaires à déterminant symétrique gauche et la théorie générale du jeu," *Comptes Rendus de l'Académie des Sciences de Paris* 184: 925–927.

BOREL, É. et al. 1938, *Traité du calcul des probabilités et de ses applications, vol. 4.2. Applications diverses et conclusions: Applications aux jeux de hasard*, Paris, Gauthier-Villars.

BOURBAKI, N. 1948, "L'architecture des mathématiques," in *Les grands courants de la pensée mathématique*, F. Le Lionnais (ed.), Paris, Cahiers du Sud: 35–47.

BRIGAGLIA A., and G. MASOTTO 1982, *Il Circolo matematico di Palermo*, Bari, Edizioni Dedalo.

BRODY, A. 1989, "Economics and thermodynamics," in Dore, Chakravarty, Goodwin, eds. 1989: 141–148.

BRÓDY, F., and T. VALMOS, eds. 1995, *The Neumann compendium*, World Scientific Series in 20^{th} century mathematics, No. 1, Singapore-River Edge, N.J., World Scientific.

BROUWER, L.E. J. 1910, "Über eineindeutige stetige Transformationen von Flächen in Sich," *Mathematische Annalen* 67: 176–180.

BROWN, L.M., and H. RECHENBERG 2005, "P.A.M. Dirac (1930) and J. von Neumann (1932), books on quantum mechanics," in Grattan-Guinness, ed. 2005: 882–900.

BUSH, V. 1945. *Science, the endless frontier: A report to the President on a program for postwar scientific research*, Washington, D.C. (reprint 1960, Washington, D.C., National Science Foundation)

CASARI, E. 1964, *Questioni di filosofia della matematica*, Milano, Feltrinelli.

CASARI, E., ed. 1973, *La filosofia della matematica del Novecento*, Firenze, La Nuova Italia.

CHAKRAVARTY, S. 1989, "John von Neumann's model of an expanding economy: an essay in interpretation," in Dore, Chakravarty, Goodwin, eds. 1989: 69–81.

CHAMPERNOWNE, D.G. 1945, "A note on J. v. Neumann's article on 'A model of economic equilibrium'," *Review of Economic Studies* 13: 10–18.

CHAPLIN, V. 1958, "Princeton and mathematics: A notable record," *Princeton Alumni Weekly* May 9: 6–15.

CINI, M. 1985, "The context of discovery and the context of validation: The proposals of von Neumann and Wiener in the development of 20th century physics," *Rivista di Storia della Scienza* 2: 99–122.

CORRY, L. 1992, "Nicholas Bourbaki and the concept of mathematical structure," *Synthèse* 22: 315–348.

CORRY, L. 1996, *Modern algebra and the rise of mathematical structures*. Basel, Birkhäuser.

CORRY, L. 1997, "The origins of eternal truth in modern mathematics: Hilbert to Bourbaki," *Science in Context* 10: 253–296.

CORRY, L. 2004, *David Hilbert and the axiomatization of physics* (1898–1918): *from Grundlagen der Geometrie to Grundlagen der Physik*, Dordrecht, Kluwer.

COURANT R., and D. HILBERT 1924–1937, *Methoden der mathematischen Physik*, Die Grundlehren der mathematischen Wissenschaften in Einzeldarstellungen, Nos. 12–48, Berlin, Springer Verlag (2^{nd} edition, 1931; English translation *Methods of mathematical physics*, 2 vols., New York, Interscience Publishers, 1953–1962).

COURANT, R. and H. ROBBINS 1941, *What is mathematics? An elementary approach to ideas and methods*, New York, Oxford University Press.

COURNOT, A. A. 1838, *Recherches sur les principes mathématiques de la théorie des richesses*, Paris, Hachette (English translation *Mathematical principles of the theory of wealth*, commentaries by I. Fisher, F.Y. Edgeworth, San Diego, James & Gordon, 1995).

DANTZIG, G.B. 1951a, "The programming of interdependent activities: mathematical model," in Koopmans, ed. 1951: 19–32.

DANTZIG, G.B. 1951b, "Maximization of a linear function of variables subject to linear inequalities," in Koopmans, ed. 1951: 339–347.

DANTZIG, G.B. 1963, *Linear programming and extensions*, Princeton, N.J., Princeton University Press.

DANTZIG, G.B. 1991, "Linear programming. The story about how it began," in Lenstra et al. ed. 1991: 19–31.

DANTZIG, G.B., and A. F. VEINOTT, eds. 1968, *Mathematics of the decision sciences*, Providence, R.I., American Mathematical Society.

DAVIS, PH. 2003, "SIAM Conference on computational science and engineering: John von Neumann at 100: SIAM Celebrates a Rich Legacy," *SIAM News* 36(4).

DEBREU, G. 1959, *Theory of Value. An Axiomatic Analysis of Economic Equilibrium*, Cowles Foundation Monograph, No. 17, New Haven, CT, Yale University Press.

DELL'AGLIO, L. 1995, "Divergences in the history of mathematics: Borel, von Neumann and the genesis of game theory," *Rivista di Storia della Scienza* 3: 1–46.

DE MARIA, M., and C. CATTANI 1989, "The 1915 epistolary controversy between Einstein and Tullio Levi-Civita," in *Einstein and the history of general relativity*, D. Howard, and J. Stachel (eds.), Boston-Basel-Berlin, Birkhäuser: 175–200.

DETLEFSEN, M. 1986, *Hilbert's program*, Dordrecht, Reidel.

DIEUDONNÉ, J. 1939, "Les méthodes axiomatiques modernes et les fondements des mathématiques," *Revue Scientifique* 77: 224–232.

DIEUDONNÉ, J. 1951, "L'axiomatique dans les mathématiques modernes," in *Congrès International de Philosophie des Sciences. Paris, 1949*, Paris, Hermann: 47–53.

DIEUDONNÉ, J. 1970, "The work of Nicholas Bourbaki," *American Mathematical Monthly* 77: 134–145.

DIEUDONNÉ, J. 1976, "Von Neumann, Johann (or John)," in *Dictionary of Scientific Bibliography*, C. Gillispie (ed.), New York, Scribners : ad vocem.

DIEUDONNÉ, J. 1977, *Panorama des mathématiques pures: le choix bourbachique*, Paris, Gauthier-Villars (English translation *A panorama of pure mathematics, as seen by N. Bourbaki*, New York, Academic Press, 1982).

DORE, M. 1989, "The legacy of John von Neumann," in Dore, Chakravarty, Goodwin, eds. 1989: 83–99.

DORE, M., S. CHAKRAVARTY, and R. GOODWIN, eds. 1989, *John von Neumann and modern economics*. Oxford, Clarendon Press.

DUREN, P., ed. 1988, *A century of mathematics in America*, 3 vols., Providence, R.I., American Mathematical Society.

EDWARDS, P.N. 1996, *The closed world: Computers and the politics of discourse in cold war America*, Cambridge, MA, The MIT Press.

ENRIQUES, F. 1920, "La evolución del concepto de la geometría y la escuela italiana durante los últimos cincuenta años," *Revista Matemática Hispano-Americana* 2: 1–17.

FADDEEV, L.D. 1990, "Mathematician's view on the development of physics," in: *Frontiers in physics, high technology, and mathematics*, H.A. Cerdeira, and S. O. Lundqvist (eds.), Singapore-River Edge, N.J., World Scientific Publications: 238–246.

FADDEEV, L.D. 1995, 40 *years in mathematical physics*, Singapore-River Edge, N.J., World Scientific Publications.

FEFERMAN, S. 1993, *The development of programs for the foundations of mathematics in the first third of the* 20*th century*, Roma, Istituto della Enciclopedia Italiana, preprint.

FEJÉR, L. 1900, "Sur les fonctions bornées et intégrables," *Comptes Rendus de l'Académie des Sciences de Paris* 131: 984–987.

FEJÉR, L. 1902, "Investigations in the theory of Fourier series (in Hung.)," *Mathematikai és Physikai Lapok* 15: 48–68, 97–123.

FEJÉR, L. 1904, "Untersuchungen über Fouriersche Reihen," *Mathematische Annalen* 58: 51–69.

FERDINANDY, M. DE 1967, *Historia de Hungría*, Madrid, Alianza Editorial.

FERREIRÓS, J. 1999, *Labyrinth of thought: A history of set theory and its role in modern mathematics*, Basel, Birkhäuser.

FINKIELKRAUT, A. 2005, *Nous autres, modernes*, Paris, Ellipses/École Polytechnique.

FISCHER, E. 1907a, "Sur la convergence en moyenne," *Comptes Rendus de l'Académie des Sciences de Paris* 144: 1022–1024.

FISCHER, E. 1907b, "Applications d'un théorème sur la convergence en moyenne," *Comptes Rendus de l'Académie des Sciences de Paris* 144: 1148–1151.

FISCHER, G. et al., eds. 1990, *Ein Jahrhundert Mathematik* 1890–1990. *Festschrift zum Jubiläum der DMV*, Braunschweig, Vieweg.

FITZPATRICK, A., S. BERKOVICH, and T. KAZOKOVA 2006, "MESM and the beginning of the computer era in the Soviet Union," *Annals of the History of Computing* 28 (3): 4–16.

FORMAN, P. 1973, "Scientific internationalism and the Weimar physicists. The ideology and its manipulation in Germany after World War I," *Isis*, 64: 151–180.

FRÉCHET, M. 1953a, "Émile Borel, initiator of the theory of psychological games and its application," *Econometrica* 21, 95–96.

FRÉCHET, M. 1953b, "Commentary on the three notes of Émile Borel," *Econometrica* 21: 118–127.

FRIEDRICHS, K.O. 1980, "Von Neumann's Hilbert space theory and partial differential equations," *SIAM Review* 22: 486–493.

GALISON, P. 1994, "The ontology of the enemy: Norbert Wiener and the cybernetic vision," *Critical inquiry* 21: 228–266.

GALISON, P., and B. HEVLY, eds. 1991, *Big science: The growth of large-scale research*, Stanford, Cal., Stanford University Press.

GAROLA, C., and A. ROSSI, eds. 2000, *The foundations of quantum mechanics: historical analysis and open questions*, Singapore-River Edge, N.J., World Scientific.

GASS, S. I., and A. A. ASSAD 2006, *An annotated timeline of operations research: An informal history*, New York, Kluwer Academic Publishers.

GAY, P. 1968, *Weimar culture*, New York, Harper & Row.

GERÖ, A. 1995, *Modern Hungarian society in the making. An unfinished experience*, Budapest, Central European University Press/London, Oxford University Press.

GERÖ, A. 2007, *The Jewish Criterion in Hungary*, New York, Columbia University Press.

GEROVITCH, S. 2002, *From newspeak to cyberspeak: a history of soviet cybernetics*, Cambridge, MA, MIT Press.

GLIMM, J., J. IMPAGLIAZZO, and I. SINGER, eds. 1990, *The legacy of John von Neumann*, Proceedings of Symposia in Pure Mathematics, No. 50, Providence, R.I., American Mathematical Society.

GÖDEL, K. 1931, "Über formal unentscheidbare Sätze der *Principia mathematica* und verwandter Systeme I," *Monatshefte für Mathematik und Physik* 38: 173–198 (English translation in *Collected Works*, vol. 1 *Publications* 1929–1936, S. Fefferman et al. (eds.), New York, Oxford University Press, 1983).

GOLDSTINE, H. 1977, *A history of numerical analysis from the* 16*th through the* 19*th century*, New York, Springer.

GOODCHILD, P. 2004, *Edward Teller: The real Dr. Strangelove*, Cambridge, MA, Harvard University Press.

GRATTAN-GUINNESS, I., ed. 1994, *Companion encyclopedia of the history and philosophy of the mathematical sciences*, 2 vols., London-New York, Routledge.

GRATTAN-GUINNESS, I., ed. 2005, *Landmark writings in Western mathematics* 1640–1940, Amsterdam, Elsevier.

GUILLAUME, G., and E. GUILLAUME 1932, *Sur les fondements de l'économique rationnelle*, Paris, Gauthier-Villars.

GUILLAUME, G., and E. GUILLAUME 1937, *L'économique rationnelle: de ses fondements aux problèmes actuels*, Paris, Hermann.

HAAR, A. 1933, "Der Massbegriff in der Theorie der kontinuierlichen Gruppen," *Annals of mathematics* 34: 147–169.

HADAMARD, J. 1898, "Les surfaces à courbure opposées et leurs lignes géodésiques," *Journal de mathématiques pures et appliquées* 4: 27–73.

HAHN, H., O. NEURATH, and R. CARNAP 1929, *Wissenschaftliche Weltauffassung. Der Wiener Kreis*, Wien, A. Wolf. Hrsg. vom Verein Ernst Mach (English translation "The scientific conception of the world: The Vienna Circle," in Neurath, O. 1973, *Empirism and sociology*, M. Neurath and R.S. Cohen (eds.) Dordrecht-Boston, D. Reidel: 299).

HALMOS, P. 1958, "Von Neumann on measure and ergodic Theory," *Bulletin of the American Mathematical Society* 64: 87–94.

HALMOS, P. 1973, "The legend of John von Neumann," *American Mathematical Monthly* 80: 382–394.

HALPERIN, I. 1990, "The extraordinary inspiration of John von Neumann," in Glimm, Impagliazzo, and Singer, eds. 1990: 15–17.

HANÁK, P. 1998, *The garden and the workshop: essays on the cultural history of Vienna and Budapest*, Princeton, N.J., Princeton University Press.

HEIMS, S.J. 1980, *John von Neumann and Norbert Wiener. From Mathematics to the Technologies of Life and Death*, Cambridge, MA, MIT Press.

HEIMS, S.J. 1991, *The Cybernetics Group*, Cambridge, MA, MIT Press.

HERSH, R., and V. JOHN-STEINER 1993, "A visit to Hungarian mathematics," *The Mathematical Intelligencer* 15 (2): 13–26.

HILBERT, D. 1899, *Grundlagen der Geometrie*, Leipzig, Teubner (English translation *Foundations of Geometry*, La Salle, Ill., Open Court, 1902; reprint 1971).

HILBERT, D. 1902, "Sur les problèmes futurs des mathématiques," in *Compte Rendu du Deuxième Congrès International des Mathématiciens*, E. Duporcq (ed.), Paris, Gauthier-Villars, vol. 1: 58–114 (Kraus reprint 1967; original German text, "Mathematische Probleme" in Hilbert 1932–35, vol. 3: 290–329).

HILBERT, D. 1918, "Axiomatisches Denken," *Mathematische Annalen* 78: 405–415.

HILBERT, D. 1931, "La connaissance de la nature et la logique," *L'Enseignement Mathématique* 30: 22–33.

HILBERT, D. 1932–35, *Gesammelte Abhandlungen*, 3 vols., Berlin: Springer.

HILBERT, D., and W. ACKERMANN 1928, *Grundzüge der theoretischen Logik*, Berlin, Springer.

HILBERT, D., and P. BERNAYS 1934–1939, *Grundlagen der Mathematik*, 2 vols., Berlin, Springer.

HODGES, A. 1983, *Alan Turing: The Enigma*, New York, Simon & Schuster,

HORVÁTH, Z. 1966, *Die Jahrhundertwende in Ungarn. Geschichte der zweiten Reformgeneration* 1896–1914, Budapest, Corvina Verlag.

HOUNSHELL, D. 1997, "The Cold War, RAND, and the generation of knowledge, 1946–1962," *Historical Studies on the Physical and Biological Sciences* 27, 237–267.

HUGHES, TH. P. 1971, *Elmer Sperry: Inventor and engineer*, Baltimore, MD Johns Hopkins University Press.

HUGHES, TH. P. 1983, *Networks of power: electrification in Western society*, Baltimore, MD, Johns Hopkins University Press.

HUGHES, TH. P. 1998, *Rescuing Prometheus. Four monumental projects that changed the modern world*, New York: Vintage Books.

HUGHES, A., and TH. P. HUGHES, eds. 2000, *Systems, experts, and computers: The systems approach in management and engineering, World War I and after*, Cambridge, MA, MIT Press.

INGRAO, B., and G. ISRAEL 1990, *The invisible hand. Economic equilibrium in the history of science*, Cambridge, MA., MIT Press.

ISRAEL, G. 1977, "Un aspetto ideologico della matematica contemporanea: il 'bourbakismo'," in *Matematica e Fisica: struttura e ideologia*, E. Donini, A. Rossi and T. Tonietti (eds.), Bari, De Donato: 35–70.

ISRAEL, G. 1978, "La matematica assiomatica e il 'bourbakismo'," in *I fondamenti della matematica dall'Ottocento ad oggi*, E. Casari, G. Israel, and F. Marchetti (eds.), Firenze, Guaraldi: 49–67.

ISRAEL, G. 1984, "Vito Volterra: un fisico matematico di fronte ai problemi della fisica del Novecento," *Rivista di Storia della Scienza* 1: 39–72.

ISRAEL, G. 1988, "The contribution of Volterra and Lotka to the development of modern biomathematics," *History and Philosophy of the Life Sciences* 10: 37–49.

ISRAEL, G. 1991, "Volterra's 'analytical mechanics' of biological associations," *Archives Internationales d'Histoire des Sciences* 41: 57–104, 306–351.

ISRAEL, G. 1992, "L'histoire du principe philosophique du déterminisme et ses rencontres avec les mathématiques," in *Chaos et déterminisme*, A. Dahan Dalmedico, J.-L. Chabert, and K. Chemla (eds.), Paris, Éditions du Seuil: 249–273.

ISRAEL, G. 1993a, "The two paths of the mathematization of the social and economic sciences. The decline of the 'mathématique sociale' and the beginnings of mathematical economics at the turn of the eighteenth century," *Physis, Rivista Internazionale di Storia della Scienza* 30: 27–78.

ISRAEL, G. 1993b, "The origins of mathematical modelling in biology: A revival of mechanical reductionism and darwinism," *Science in context* 6 (2): 469–509.

ISRAEL, G. 1996a. *La mathématisation du réel*, Paris, Editions du Seuil (Italian translation *La visione matematica della realtà*, Roma-Bari, Laterza, 1996, 3^{rd} ed. 2003).

ISRAEL, G. 1996b, "'Administrer c'est calculer': due 'matematici sociali' nel declino dell'Età dei Lumi," *Bollettino di Storia delle Scienze Matematiche* 16: 241–314.

ISRAEL, G. 1997, "Il principio di minima azione e il finalismo in meccanica," *Le Scienze* 58: 70–76.

ISRAEL, G. 2001, "L'idéologie de la toute puissance de la science. La constitution des champs disciplinaires," in *L'Europe des sciences. Constitution d'un espace scientifique*, M. Blay and E. Nicolaïdis (eds.), Paris, Éditions du Seuil: 135–162.

ISRAEL, G. 2002, "The two faces of mathematical modelling: Objectivism vs. subjectivism, simplicity vs. complexity," in *The application of mathematics to the sciences of nature. Critical moments and aspects*, P. Cerrai, P. Freguglia, and C. Pellegrini (eds.), New York, Kluwer Academic/Plenum Publishers: 233–244.

ISRAEL, G. 2004a, "Technological innovation and new mathematics: van der Pol and the birth of non-linear dynamics," in Lucertini, Millán Gasca, and Nicolò eds. 2004: 52–78.

ISRAEL, G. 2004b, "Oltre il mondo inanimato: la storia travagliata della matematizzazione dei fenomeni biologici e sociali," *Bollettino dell'Unione Matematica Italiana* 8 (7-B): 275–304.

ISRAEL, G. 2004c, *La macchina vivente*, Torino, Bollati Boringhieri.

ISRAEL, G. 2004d, "Science and the Jewish question in the twentieth century: The case of Italy and what it shows," *Aleph, Historical Studies in Science and Judaism* 4: 191–261.

ISRAEL, G. 2005, "The science of complexity. Epistemological problems and perspectives," *Science in Context* 18 (3): 1–31.

ISRAEL, G., and A. MILLÁN GASCA 2002, *The Biology of Numbers. The Correspondence of Vito Volterra on Mathematical Biology*, Basel-Boston-Berlin, Birkhäuser Verlag.

ISRAEL, G., and P. NASTASI 1998, *Scienza e razza nell'Italia fascista*, Bologna, Il Mulino.

JAMMER, M. 1966, *The conceptual development of quantum mechanics*, New York, McGraw-Hill.

JÁSZI, O. 1929, *The dissolution of the Habsburg monarchy*, Chicago, University of Chicago Press.

KALMÁR, L. 1928, "Zur Teorie der abstrakten Spiele," *Acta Scientiarum Mathematicarum Szeged* 4, 65–85.

KAY, L. 2000a, "How a genetic code became an information system," in Hughes and Hughes, eds. 2000: 463–491.

KAY, L. 2000b, *Who wrote the Book of Life? A history of the genetic code*, Stanford, Stanford University Press.

KEVLES, D.J. 1995, *The physicists: the history of a scientific community in modern America*, Cambridge, MA, Harvard University Press.

KLEIN, J. 1999, *Controlling gunfires, inventories, and expectations with the exponentially weighted moving average*, Staunton, VA, Mary Baldwin College, preprint.

KLEIN, J. 2000, "Economics for a client: the case of statistical quality control and sequential analysis," *History of Political Economy* 32 (supplement *Toward a History of Applied Economics*, R.E. Backhouse and J. Biddle, eds.): 27–69.

KLEIN, J. 2001a, *Post-war economics and shotgun weddings in control engineering*, Staunton, VA, Mary Baldwin College, preprint.

KLEIN, J. 2001b, "Reflections from the Age of Economic Measurement," *History of Political Economy* 33: 111–136.

KLINE M. 1972, *Mathematical thought from ancient to modern times*, New York, Oxford University Press.

KLINE M. 1980, *Mathematics. The Loss of Certainty*, New York, Oxford University Press.

KLINE, R. 1995, "Constructing 'technology' as 'applied science'. Public rhetoric of scientists and engineers in the United States, 1880–1945," *Isis* 86: 194–221.

KNOBLOCH, E., and D. ROWE, eds. 1994, *The history of modern mathematics. Vol. III, Images, ideas, and communities*, Boston-San Diego, Academic Press.

KOESTLER, A. 1952, *Arrow in the blue: an autobiography*, London, Collins.

KÖNIG, D. 1927, "Über eine Schlussweise aus dem Endlichen ins Unendliche, *Acta Scientiarum Mathematicarum Szeged* 3: 121–130.

KÖNIG, D. 1936, *Theorie der Endlichen und Unendlichen Graphen, Kombinatorische Topologie der Streckenkomplexe*, Leipzig, Akademische Verlagsgesellschaft M.b.H. (reprint New York, Chelsea Pub. Co., 1950; English translation *Theory of finite and infinite graphs*, with commentary by W.T. Tutte. Boston, Birkhäuser, 1990).

KÖNIG, G. 1914, *Neue Grundlagen der Logik, Arithmetik und Mengenlehre*, Leipzig, Veit.

KOOPMANS, T.C. et al., eds. 1951, *Activity analysis of production and allocation. Proceedings of a Conference*, Cowles Foundation Monographs, No. 8, New York, John Wiley & Sons (8th ed., New Haven/London, Yale University Press, 1976).

KOYRÉ, A. 1962, *Introduction à la lecture de Platon, suivie d'entretiens sur Descartes*, Paris, Gallimard.

KUHN, H.W., and A. W. TUCKER, eds. 1950–1953, *Contributions to the theory of games*, 2 vols., Princeton, N.J., Princeton University Press.

KUHN, H.W., and A. W. TUCKER 1958, "John von Neumann's work in the theory of games and mathematical economics," *Bulletin of the American Mathematical Society* 64: 100–122.

LACKI, J. 2000, "The early axiomatizations of quantum mechanics: Jordan, von Neumann and the continuation of Hilbert's program," *Archives for the History of Exact Sciences* 54, 279–318.

LE LIONNAIS, F., ed. 1948, *Les grands courants de la pensée mathématique*. Paris, Cahiers du Sud (English translation *Great currents of mathematical thought*, 2 vols., New York, Dover, 1971).

LEGENDI T., and T. SZENTUVANYI 1983, *Leben und Werk von John von Neumann*, Mannheim-Wien-Zurich, Bibliographisches Institut.

LENSTRA, J.K., A.H.G. RINNOOY KAN, and A. SCHRIJVER, eds. 1991, *History of mathematical programming. A collection of personal reminiscences*, Amsterdam, CWI/North Holland.

LEONARD, R.J. 1992, "Creating a context for game theory," in Weintraub, ed. 1992: 29–76.

LEONARD, R.J. 1995, "From parlor games to social science: von Neumann, Morgenstern and the creation of game theory 1928–1944," *Journal of Economic Literature* 33(2): 730–761.

LEONARD, R.J. 1998, "Ethics and the excluded middle: Karl Menger and social science in interwar Vienna," *Isis* 89: 1–26.

LEONARD, R.J. 2004a, "Structures sous tension: théorie des jeux et psychologie sociale à la Rand," in *Les Sciences pour la guerre* 1940–1960, A. Dahan, and D. Pestre, (eds.), Paris, Éditions de l'École des Hautes Études en Sciences Sociales: 83–127.

LEONARD, R.J. 2004b, "'Between worlds', or an imagined reminiscence by Oskar Morgenstern about equilibrium and mathematics in the 1920's," *Journal of the History of Economic Thought* 26 (3): 285–310.

LESLIE, S.W. 1993, *The cold war and American science: The military-industrial-academic complex at M.I.T. and Stanford*, New York, Columbia University Press.

LEVI-CIVITA, T. 1919, "Come potrebbe un conservatore giungere alla soglia della nuova meccanica," *Rendiconti del Seminario Matematico della Facoltà di Scienze della Regia Università di Roma* 5: 10–28.

LEVIN, M.R., ed. 1999, *Cultures of control*, Amsterdam, Harwood.

LOTKA, A.J. 1925, *Elements of Physical Biology*, Baltimore, Williams &Wilkins company.

LOTKA, A.J. 1956. *Elements of mathematical biology*, New York, Dover.

LUCE, R.D., and H. RAIFFA 1957, *Games and decisions. Introduction and critical survey*, New York, Dover.

LUCERTINI, M., A. MILLÁN GASCA, and F. NICOLÒ, eds. 2004, *Technological concepts and mathematical methods in the evolution of modern engineering systems. Controlling, managing, organizing*, Basel, Birkhäuser.

LUKACS, L. 1988, *Budapest 1900*, New York, Weidenfeld & Nicolson.

MACH, E. 1883, *Die Mechanik in ihrer Entwicklung, historisch-kritisch dargestellt*. Leipzig, Brokhaus (English translation from the 2^{nd} ed., *The science of mechanics; a critical and historical exposition of its principles*, Chicago, The Open Court Publishing Co, 1893; 6th ed., with revisions through the 9th German ed. and new introduction by K. Menger, La Salle, Ill., Open Court Pub. Co., 1960).

MCCULLOCH W., and W. PITTS 1943, "A logical calculus of the ideas immanent in nervous activity," *Bulletin of Mathematical Biophysics* 5: 115–133.

MACRAE, N. 1992, *John von Neumann*, New York, Pantheon Books.

MCCAGG, W.O. 1972, *Jewish nobles and geniuses in modern Hungary*, New York, Columbia University Press.

MCCAGG, W.O. 1997, *A history of Habsburg Jews* 1670–1918, Bloomington-Indianapolis, Indiana University Press.

MALINOVSKY, B.N. 2007, *Pioneers of Soviet computing*, E-book, copyright Anna Fitzpatrick, http://sovietcomputing.com

MARX, G. 1997, *The voice of the martians. Hungarian scientists that shaped the 20th century in the West*, Budapest, Akádemiai Kiadó: 364–365; 3rd ed. 2001.

MASHAAL, M. 2006, *Bourbaki: A secret society of mathematicians*, Providence, R.I., American Mathematical Society.

MAYNTZ, R., and TH. P. HUGHES, eds. 1988, *The development of large technical systems*, Frankfurt a. M., Campus Verlag/Boulder (Col.),Westview Press.

MENGER, K. 1934. *Moral, Wille und Weltgestaltung. Grundlegung für Logik der Sitten*. Wien, Springer (English translation *Morality, decision, and social organization: toward a logic of ethics*, Dordrecht-Boston, D. Reidel Pub. Co., 1974).

MENGER K. 1973, "Austrian marginalism and mathematical economics," in *Carl Menger and the Austrian school of economics*, J.R. Hicks and W. Weber (eds.), Oxford, Clarendon Press: 38–60.

METROPOLIS N., J. HOWLETT, and G.-C. ROTA 1980, *A history of computing in the twentieth century*, New York, Academic Press.

MILLÁN GASCA, A. 2000, "Matematica," in *Appendice 2000. Sesta Appendice della Enciclopedia Italiana di Lettere, Arti e Scienze*, Roma, Istituto della Enciclopedia Italiana: ad vocem.

MILLÁN GASCA, A. 2003a, "Organization and mathematics: A look into the prehistory of industrial engineering," in Lucertini, Millán Gasca and Nicolò, eds. 2004: 21–50.

MILLÁN GASCA, A. 2003b, "La aplicación de las matemáticas a los problemas de administración y organización: antecedentes históricos," *Llull, Revista de la Sociedad Española de Historia de las Ciencias y de las Técnicas* 57 : 929–961.

MILLÁN GASCA, A. 2003b, "Early approaches to the management of complexity in engineering systems," in *Determinism, Holism, and Complexity*, V. Benci et al. (eds.), New York-Boston-Dordrecht-London-Moscow, Kluwer Academic/Plenum Publishers: 349–357.

MILLÁN GASCA, A. 2006, *Fabbriche, sistemi, organizzazioni. Storia dell'ingegneria industriale*, Milano, Springer Verlag.

MINDELL, D. 2002, *Between human and machine: Feedback, control, and computing before cybernetics*, Baltimore, MD, Johns Hopkins University Press.

MIROWSKI, P. 1992, "What were von Neumann and Morgenstern trying to accomplish?," in Weintraub, ed. 1992: 113–147.

MIROWSKI, P. 1999, "Cyborg agonistes: Economics meets operations research in mid-century," *Social Studies of Science* 29: 685–718.

MIROWSKI, P. 2002, *Machine dreams. Economics becomes a cyborg science*, Cambridge-New York, Cambridge University Press.

MORAVIA, S. 1974, *Il pensiero degli Idéologues. Scienza e filosofia in Francia* (1780–1815), Firenze, La Nuova Italia.

MORAVIA, S. 1986, *Il tramonto dell'Illuminismo. Filosofia e politica nella società francese* (1770–1810), Roma-Bari, Laterza.

MORGENSTERN, O. 1936, "Logistik und Sozialwissenschaften," *Zeitschrift für Nationalökonomie* 6: 1–24 (English translation in Schotter, ed. 1976: 185–217).

MORGENSTERN, O. 1973, "Game theory," *Dictionary of the History of Ideas*, New York, Scribner: *sub voce*.

MORGENSTERN O. 1976, "The collaboration between O. Morgenstern and J. von Neumann on the theory of games," *Journal of Economic Literature* 14: 805–816.

MOY, T. 2001, *War machines: transforming technologies in the U.S. Military*, 1920–1940, College Station, Texas A&M University Press.

MUMFORD, D. 1998, "Trends in the profession of mathematics," in *The Berlin Intelligencer* (*International Congress of Mathematicians, Berlin, August* 1998), Berlin-Heidelberg, Springer: 2–5.

NAGY F., ed. 1987, *Neumann János és a "Magyar Titkok": a dokumentumok tükrében*, Budapest, Országos Müszaki Információs Központ és Könyvtár.

NAGY, D., F. NAGY, and P. HORVATH 1987, *The computer, the brain, and the missing link* (*In memoriam John von Neumann*), Budapest, Országos Müszaki Információs Központ és Könyvtár.

NASAR, S. 1998, *A beautiful mind*, New York, Simon and Schuster.

NEURATH, O. 1932–33 "Protokollsätze," *Erkenntnis* 3: 204–214 (English translation in Ayer, ed. 1959: 199–208)

NEURATH, O. 1936, "Une encyclopédie internationale de la science unitaire," in *Actes du Congrès International de Philosophie Scientifique, Sorbonne, Paris 1935*. Paris, Hermann, vol. II: 54–59.

OWENS, L. 1989, "Mathematicians at war: Warren Weaver and the Applied Mathematics Panel, 1942–1945," in Rowe, and McCleary, eds. 1989: 287–306.

PAPP, K. 1999, "Hungarian schools: past, present, and future," *Fizikai Szemle* 49(5).

PARETO, V. 1906, *Manuale di economia politica: con una introduzione alla scienza sociale*, Piccola biblioteca scientifica, No. 13, Milano, Società editrice libraria (English translation, *Manual of political economy*, A.S. Schwier and A.N. Page (eds.), New York, A.M. Kelley, 1971).

PARMET, H.S. 1972, *Eisenhower and the American crusade*, New York, Macmillan.

PARSHALL, K.H. 1996, "How we got where we are: An international overview of mathematics in national contexts (1875–1900)," *Notices of the American Mathematical Society* 43, 287–296.

PARSHALL, K.H., and A.C. RICE, eds. 2002, *Mathematics unbound: the evolution of an international mathematical research community*, 1800–1945, Providence, R.I., American Mathematical Society.

PARSHALL, K. H., and D. E. ROWE 1988, "American mathematics comes of age: 1875–1900," in Duren, ed. 1988: 3–28.

PARSHALL, K.H., and D. E. ROWE 1994, "The United States of America, and Canada," in Grattan-Guinness, ed. 1994: 1512–1525.

PEANO, G. 1889, *Arithmetices principia, nova metodo exposita*, Torino, Bocca.

PICKERING, A. 1995, "Cyborg history and the WWII regime," *Perspectives in science* 3: 1–45.

PIER, J.-P., ed. 1994, *Development of mathematics*, 1900–1950, Basel, Birkhäuser.

PIER, J.-P., ed. 2000, *Development of mathematics*, 1950–2000, Basel, Birkhäuser.

PITCHER, E. 1988, *A history of the second fifty years, American Mathematical Society* 1939–1988, Providence, R.I., American Mathematical Society.

POINCARÉ, H. 1892–99, *Les méthodes nouvelles de la mécanique céleste*, 3 vols. Paris, Gauthier-Villars (English translation *New methods of celestial mechanics*, Woodbury, N.Y., American Institute of Physics, 1993).

POINCARÉ, H. 1902. *La science et l'hypothèse*. Paris, Flammarion. (English translation in *The foundations of sciente*, New York, Garrison, N.Y., The Science Press, 1913; also Washington, D.C., University Press of America, 1982).

POLIZZI, G., ed. 1993, *Filosofia scientifica ed empirismo logico* (*Parigi,* 1935), Milano, Edizioni Unicopli.

POLYA, G. 1954*, Mathematics and plausible reasoning*, 2 vols, Princeton, N.J., Princeton University Press.

POUNDSTONE, W. 1992, *Prisoner's dilemma: John von Neumann, game theory and the puzzle of the bomb*, New York, Doubleday.

PRIGOGINE, I. 1962, *Introduction to nonequilibrium thermodynamics*. New York, Wiley-Interscience.

QUESNAY, F. 1767, "Dispotisme de la Chine," *Ephémerides du citoyen ou Bibliothèque raisonnée des Sciences Morales et Politiques* 3: 5–88.

RÉDEI, M. 1996, "Why John von Neumann did not like Hilbert space formalism of quantum mechanics (and what he liked instead)," *Studies in the History and Philosophy of Modern Physics* 27: 493–510.

RÉDEI, M. 1999, "'Unsolved problems in mathematics'. J. von Neumann's address to the International Congress of Mathematicians, Amsterdam, September 2–9, 1954," *The Mathematical Intelligencer* 21: 7–12.

RÉDEI, M. 2005, "John von Neumann on mathematical and axiomatic physics," in *The role of mathematics in physical sciences: Interdisciplinary and philosophical aspects*, G. Boniolo, P. Budinich, and M. Trobok (eds.), Dordrecht, Springer: 43–54

RÉDEI, M., ed. 2005, *John von Neumann: Selected letters*, Providence, R.I., American Mathematical Society.

RÉDEI, M., and M. STÖLTZNER, eds. 2001, *John von Neumann and the foundations of quantum physics*, Dordrecht-Boston-London, Kluwer Academic Publishers.

REES, M. 1988, "The mathematical science and World War," in Duren, ed. 1988: 275–290.

REID, C. 1973, *Hilbert*, New York, Springer Verlag.

REINGOLD, N. 1981, "Refugee Mathematicians in the United States of America, 1933–1941: Reception and Reaction," *Annals of science* 38: 313–338.

RELLSTAB, U. 1992, "New insights into the collaboration between John von Neumann and Oskar Morgenstern on the theory of games and economic behavior," in Weintraub, ed. 1992: 77–93.

RICHENHAGEN, G. 1985, *Carl Runge (1856–1927): von der reinen Mathematik zur Numerik*, Göttingen, Vandenhoeck & Ruprecht.

RIESZ, F. 1907, "Sur les systèmes orthogonaux de fonctions," *Comptes Rendus de l'Académie des Sciences de Paris* 144 : 615–619.

RIESZ, F. 1910, “Untersuchungen über Systeme integrierbarer Funktionen,” *Mathematische Annalen* 69: 449–497.

ROSENBLUETH, A., N. WIENER, and J. BIGELOW 1943, “Behaviour, purpose and teleology,” *Philosophy of science* 10: 18–24.

ROZENBLIT, M.L. 2001, *Reconstructing a national identity: The Jews of Habsburg Austria during World War I*, New York, Oxford University Press.

ROWE, D.E. 1986, “‘Jewish mathematics’ at Göttingen in the era of Felix Klein,” *Isis* 77: 422–449.

ROWE, D.E. 1989, “Klein, Hilbert, and the Göttingen mathematical tradition,” *Osiris* 5: 186–213.

ROWE, D.E. 1994, “The philosophical views of Klein and Hilbert,” in *The intersection of history and mathematics*, S. Chikara, S. Mitsuo, and J.W. Dauben (eds.), Boston, Birkhäuser: 187–202.

ROWE, D.E. 2001, “Felix Klein as Wissenschaftspolitiker,” in *Changing images in mathematics. From the French revolution to the new millennium*, U. Bottazzini and A. Dahan (eds.), London, Routledge: 69–92.

ROWE, D.E. 2003, “Mathematical schools, communities, and networks,” In *The Cambridge history of science, vol.* 5 *Modern physical and mathematical sciences*, M.J. Nye (ed.), Cambridge, Cambridge University Press: 113–132.

ROWE, D.E., and J. MCCLEARY, eds. 1989, *The history of modern mathematics*, 2 vols., Boston, Academic Press.

RUNGE, C., and H. KÖNIG 1924, *Vorlesungen über numerisches Rechnen*, Berlin, Springer.

SAMUELSON, P.A. 1947, *Foundations of economic analysis*, Cambridge, MA, Harvard University Press.

SAMUELSON, P.A. 1989, “Revisionist view of von Neumann’s growth mode,” in Dore, Chakravarty, and Goodwin, eds. 1989: 100–122.

SCHMIDT, E. 1907, “Zur Teorie der linearen und nichtlinearen Integralgleichungen,” *Mathematischen Annalen* 63: 433–476.

SCHOTTER A., ed. 1976, *Selected economic writings of Oskar Morgenstern*, New York, New York University Press.

SCORZA, G. 1903, “A proposito del massimo di ofelimità dato dalla libera concorrenza,” *Giornale degli Economisti*, Gennaio: 41–62.

SEGAL, S. L. 2003, “War, refugees, and the creation of an international mathematical community,” in Parshall, and Rice, eds. 2003: 359–380.

SHANNON, C.E. 1958, “Von Neumann’s contributions to automata theory,” *Bulletin of the American Mathematical Society* 64 (3): 123–129.

SIEGMUND-SCHULTZE, R. 2001, *Rockefeller and the internationalization of Mathematics Between the Two World Wars. Documents and Studies for the Social History of Mathematics in the* 20*th Century*, Basel, Birkhäuser.

SMITH, J.K. 1990, "The scientific tradition in American industrial research," *Technology & Culture* 31: 121–131.

SMITHIES, F. 1959, "John von Neumann," *Journal of the London Mathematical Society* 34: 373–384.

SNOW, C.P. 1963, *The two cultures: and a second look. An expanded version of the two cultures and the scientific revolution*, Cambridge, Cambridge University Press

SOMENZI, V., ed. 1965, *Johann von Neumann, Gilbert Ryle, C.E. Shannon, Charles Sherrington, A.M. Turing, Norbert Wiener e altri. La filosofia degli automi*, Boringhieri, Torino.

STERN, N. 1981, *From ENIAC to UNIVAC*, Bedford, MA, Digital Press.

STÖLTZNER, M. 2007, "A new glimpse of John von Neumann's thought laboratory," *Studies In History and Philosophy of Science Part B: Studies In History and Philosophy of Modern Physics* 38 (4): 938–947.

SZELEZSÁN, J. 1983, "John von Neumann, 'der Rechentechniker' (sein Werk auf dem Gebiet der numerischen Methoden)," in Legendi and Szentuvanyi, eds. 1983: 115–151.

SZÉNASSY, B. 1992, *History of mathematics in Hungary until the 20th century*, New York, Springer Verlag (translated and enlarged version of *A Magyarországi Matematica Története*, Budapest, Akadémiai Kiadó, 1970).

SZILARD, L. 1929, "Über die Entropieverminderung in einem thermodynamischen System bei Eingriffen intelligenter Wesen," *Zeitschrift für Physik* 53: 840–856.

TARWATER, D., ed. 1977, *The bicentennial tribute to American mathematicians 1776–1976*, Washington, D.C., Mathematical Association of America.

TODD, J. 1974, "John von Neumann and the National Accounting Machine," *SIAM Review* 16 (4): 526–530.

The Princeton Mathematics Community in the 1930s: An Oral-History Project (located at Princeton University – Seeley G. Mudd Manuscript Library), http://www.princeton.edu/mudd/math/

TURNER, R.S. 1971, "The growth of professional research in Prussia, 1818 to 1848: Causes and context," *Historical Studies in the Physical Sciences*, 3: 137–182.

ULAM, S. 1958, "John von Neumann 1903–1957," *Bulletin of the American Mathematical Society* 64: 1–49.

ULAM, S. 1976, *Adventures of a mathematician*, New York, Scribner.

VAN DER POL B.L., and VAN DER MARK, J. 1928, "The heartbeat considered as a relaxation oscillation, and an electrical model of the heart," *The London Edinburgh and Dublin Philosophical Magazine and Journal of Science* 6: 763–775.

VAN HOVE, L. 1958, "Von Neumann's contributions to quantum theory," *Bulletin of the American Mathematical Society* 64: 95–99.

VEBLEN, O. 1937, *Geometry of complex domains: a seminar conducted by Professors Oswald Veblen and John Von Neumann*, 1935–36 (First term lectures by Professor Veblen, second term lectures by Mr. J.W. Givens, notes by Dr. A.H. Taub and Mr. J.W. Givens), Princeton, The Institute for Advanced Study, reprint 1955; University of Tokyo Press, reprint, 1970.

VILLE, J. 1938, "Sur la théorie générale des jeux où intervient l'habileté des joueurs," in Borel 1938, 105–113.

VONNEUMAN, N. 1987, *John von Neumann as seen by his brother*. Meadowbrook, Pa, private publication (available in the John von Neumann papers at the Library of Congress, box 34, folder 3).

VONNEUMANN, N. 1990, "The philosophical legacy of John von Neumann, in the light of its inception and evolution in his formative years," in Glimm, Impagliazzo, and Singer, eds. 1990: 19–24.

WAERDEN (VAN DER), B.L. 1931, *Moderne Algebra*, Berlin, Julius Springer (English translation *Modern algebra*, 2 vols., New York, F. Ungar, 1949–1950).

WALD, A. 1945, "Statistical decision functions which minimize the maximum risk," *Annals of mathematics* 46: 265–280.

WALD, A. 1950, *Statistical decision functions*, New York, Wiley.

WALRAS, L. 1909, "Économique et Mécanique," *Bulletin de la Société Vaudoise de Sciences Naturelles* 45 (reprinted in *Metroeconomica* 12 : 3–11).

WEIL, A. 1991, *Souvenirs d'apprentissage*, Basel-Boston, Birkhäuser (English translation *The apprenticeship of a mathematician*, Basel-Boston, Birkhäuser, 1992).

WEINTRAUB, E.R. 1983, "On the existence of a competitive equilibrium: 1930–1954," *Journal of Economic Literature* 21: 1–39.

WEINTRAUB, E.R. 1985, *General equilibrium analysis*, Cambridge, Cambridge University Press.

WEINTRAUB, E.R., ed. 1992, *Toward a history of game theory*, Durham, NC-London, Duke University Press.

WHITMAN, M. V. N. 1990, "John von Neumann: a personal view," in Glimm, Impagliazzo, and Singer, eds. 1990, 1–4.

WIDMANN, H. 1973, *Exil und Bildungshilfe: die deutschsprachige akademische Emigration in die Türkei nach* 1933*; mit einer Bio-Bibliographie der emigrierten Hochschullehrer*, Bern, Herbert Lang/Frankfurt, Peter Lang.

WIENER, N. 1949, *The extrapolation, interpretation and smoothing of stationary time series, with engineering applications*, Cambridge, MA, MIT Radiation Laboratory.

WIENER, N. 1950, *The human use of human beings. Cybernetics and society*, New York, Avon Books.

WIENER, N. 1956, *I am a mathematician, the later life of a prodigy*, Garden City, N.Y., Doubleday.

WIENER, N. 1961, *Cybernetics: or control and communication in the animal and the machine*, Cambridge, MA, The MIT Press (2nd ed.; 1st ed. 1948).

WIENER, N. 1964, *God & Golem, Inc. A comment on certain points where cybernetics impinges on religion*, Cambridge, MA, The MIT Press.

WIGNER, E. 1960, "The unreasonable effectiveness of mathematics in the natural sciences," *Communications in pure and applied mathematics* 13: 1–14

ZACH, R. 2005, "1931 Kurt Gödel, paper on the incompleteness theorems," in Grattan-Guinness, ed. 2005: 917–925.

ZADEH, L.A. 1965, "Fuzzy sets," *Information and Control* 8: 338–353.

ZAUBERMAN, A. 1969, "The Rapprochement between East and West in Mathematical-Economic Thought," *The Manchester School of Economic & Social Studies* 37(1): 1–21

ZAUBERMAN, A. 1975, *Differential games and other game-theoretic topics in Soviet literature: A survey*, New York, New York University Press.

ZAUBERMAN, A. 1976, *Mathematical theory in Soviet planning: Concepts, methods, techniques*, London/New York, Oxford University Press for the Royal Institute of International Affairs.

ZERMELO, E. 1908, "Untersuchungen über die Grundlagen der Mengenlehre I," *Mathematische Annalen* 65: 261–281.

ZERMELO, E. 1913, "Über eine Anwendung der Mengenlehre auf die Theorie der Schachspiele," in *Proceedings of the Fifth International Congress of Mathematicians. Cambridge, 22–28 August* 1912, E.W. Hobson and A.E.H. Love (eds.), Cambridge, Cambridge University Press, vol. 2: 501–504.

ZUBOK, V.M. 2007, *A failed empire: The Soviet Union in the Cold War from Stalin to Gorbachev*, Chapel Hill, University of Carolina Press.

ZUBOK, V.M., and C. PLESHAKOV 1996, *Inside Kremlin's Cold War: from Stalin to Khrushchev*, Cambridge, MA, Harvard University Press.

Index of Names

Dates of birth and death are indicated for individuals relevant for the scientific issues discussed in the book.

Ackermann, Wilhelm (1896–1962), 28, 29
Ahrens, Wilhelm (1872–1927), 39
Aiken, Howard (1900–1973), 85, 86, 158
Alder, Ken, 114
Althoff, Friedrich (1839–1908), 20
Arany, Dániel (1863–1945), 4
Arnold, Henry H. (1886–1950), 106, 107
Arrow, Kenneth J. (1921), 131, 133, 135
Artin, Emil (1898–1962), 81
Aspray, William, 10, 51, 73, 75, 83–85, 96, 105, 112, 115, 117, 141–145, 149–151, 157, 158, 161, 162, 175, 176
Atanasoff, John (1903–1995), 85
Atatürk, Kemal (1881–1938), 79
Aubin, David, 54
Aumann, Robert J. (1930), 131, 132
Aydelotte, Frank (1880–1956), 144
Ayer, Alfred Jules (1910–1989), 124

Baker, Keith M., x
Bamberger Fuld, Caroline, 78
Bamberger, Louis (1855–1944), 78
Bateson, G. (1904–1980), 141, 158
Beaulieu, Liliane, 53
Bellinson, H. R., 84
Bennett, Stuart, 139, 140
Bernal, John D. (1901–1971), 134
Bernays, Paul (1888–1977), 22, 28, 29
Bernkopf, M., 6
Bernstein, Felix (1878–1956), 22
Bigelow, Julian H. (1913–2003), 109, 140, 145
Birkhoff, Garrett (1911–1996), 30, 38, 81, 82, 154, 155, 175
Birkhoff, George D. (1884–1944), 81
Bjerknes, Vilhelm (1862–1951), 152
Blackett, Patrick M. S. (1897–1974), 134
Blaschke, Wilhelm (1885–1962), 16
Blumenthal, Otto (1876–1944), 22
Bôcher, Maxime (1855–1918), 82, 174
Bochner, Solomon (1899–1982), 81, 82
Bode, Heindrik W. (1905–1982), 113, 139
Bohr, Harald August (1887–1951), 17
Bohr, Niels (1885–1962), 33, 37, 88
Boltzmann, Ludwig (1844–1906), 13, 61, 80, 81, 156
Bolyai, János (1802–1860), 4
Bonaparte, Napoleon (1769–1821), x, 54, 71, 114, 132, 171
Bonhoeffer, Karl F. (1899–1957), 161
Borel, Emile (1871–1956), 41–45, 70, 71, 73
Born, Max (1882–1970), 22
Bourbaki, Nicolas, 53, 54, 56, 65

Brewster, Mike, 17
Brigaglia, Aldo, 55
Bródy, F, 176
Brouwer, Luitzen E. J. (1881–1967), 27–29, 41, 44, 47, 58, 59, 125, 126
Burali-Forti, Cesare (1861–1931), 26
Burks, Arthur W. (1915–2008), 142, 144, 149, 161, 176
Bush, Vannevar (1890–1974), 83–85, 88, 138–140, 145

Caldwell, Samuel (1904–1960), 145
Cantor, Georg (1845–1918), 13, 14, 20, 23, 25–27, 58, 59
Carathéodory, Constantin (1873–1950), 22
Carnap, Rudolf (1891–1970), 13, 29, 124, 134, 141
Carson, John R. (1887–1940), 139
Casari, Ettore, 26
Cattani, Carlo, 32
Chakravarty, Sukhamoy, 46
Chamberlain, Neville (1869–1940), 92
Charney, Jule (1917–1981), 153
Church, Alonzo (1903–1995), 143
Cini, Marcello, 38
Clausewitz, Karl (von) (1780–1831), 39
Clebsch, Alfred (1833–1872), 19, 20
Collbohm, Frank, 113
Condorcet, Jean Antoine Nicolas Caritat (Marquis de) (1743–1794), x
Corry, Leo, 54
Courant, Richard (1888–1972), 15, 22, 35, 116, 147, 151
Cournot, Antoine Augustin (1801–1877), 123
Crelle, August Leopold (1780–1855), 3

Dán, Klára, 83, 118, 174
Dantzig, George B. (1914–2005), 135–137
Debreu, Gérard (1921–2004), 131–133
Debye, Peter J. (1884–1966), 22
Dedekind, Richard (1831–1916), 23, 25
Delbrück, Max (1906–1981), 160, 161
Dell'Aglio, Luca, 41, 44
De Maria, Michelangelo, 32
Dieudonné, Jean (1906–1992), 48, 59, 60
Dirac, Paul A. (1902–1984), 35, 38
Dore, Mohammed, 46
Duhem, Pierre (1861–1916), 13
Duren, Paul, 75

Eckert, John P. (1919–1995), 96
Edwards, Paul N., 105, 115, 140
Einstein, Albert (1879–1955), 13, 15, 25, 31–33, 66, 88, 167, 168
Eisenhower, Dwight D., 90, 91, 103, 104, 114, 119
Enriques, Federigo (1871–1946), 20
Eötvös, Loránd (1848–1919), 4
Euclid, 24, 54

Faddeev, Ludwig D., 38
Feferman, Solomon, 26
Fejér, Lipót (1880–1959), 4–7, 12, 14, 36
Fekete, Mihály (1886–1957), 5, 7, 11, 173
Feller, William (1906–1970), 22
Fellner, William J. (1905–1983), 10, 11
Ferdinandy, M. de, 2
Fermi, Enrico (1901–1954), 12, 68, 174
Fermi, Laura, 12
Ferreirós, José, 26
Fine, Henry B. (1858–1928), 77
Fine, Paul, 119

Finkielkraut, Alain, 133
Fischer, Ernst (1875–1954), 6, 34, 37
Fitzpatrick, Anne, 163
Flexner, Abraham (1866–1959), 78, 126, 127
Flood, Merrill (1908–1991), 108, 113, 135
Forman, Paul, 21
Forrester, Jay W. (1918), 109, 111
Foster Dulles, John (1988–1959), 90
Fourier, Joseph Baptiste (1768–1830), 5, 140
Fraenkel, Abraham A. (1891–1965), 14, 26, 27
Frank, Lawrence K. (1890–1955), 158
Fréchet, Maurice (1878–1973), 6, 45
Frege, Gottlob (1838–1925), 13, 27
Fricke, Robert K. E. (1901–1982), 22
Friedrichs, Kurt (1901–1982), 151

Gábor, Dénes (1900–1979), 11, 13
Galilei, Galileo (1564–1642) , ix, 50, 66, 73, 169
Galison, Paul, 140
Galois, Evariste (1811–1832), 53
Gamow, George (1904–1968), 65, 160
Garola, Claudio, 32
Gardner, Trevor, 112
Gauss, Carl Friedrich (1777–1855), 20, 147
Gay, Peter, 21
Gergonne, Joseph (1772–1859), 3
Gerö, A., 1, 2
Gerovitch, Slava, 163
Glimm, James, 9, 12, 68, 146, 175
Gödel, Kurt (1906–1978), 13, 29–31, 51, 56, 58, 59, 70, 143
Goldstine, Herman H. (1913–2004), 86, 104, 143, 144, 147–150
Gömbös, Gyula, 12
Goodwin, Richard, 46
Gordan, Paul A. (1837–1912), 22
Grattan-Guinness, Ivor. 4, 26
Groves, Leslie R. (1886–1970), 86
Guillaume, Edouard (1861–1938), 126, 127
Guillaume, Georges, 126, 127

Haar, Alfréd (1885–1933), 5–7, 81
Haber, Fritz (1868–1934), 13
Hadamard, Jacques (1865–1963), 54, 61
Hahn, Hans (1879–1934), 13, 124, 134
Halmos, Paul A. (1916–2006), 11, 80–82, 175
Halperin, Israel (1911–2007), 80
Hamilton, William R. (1805–1865), 32, 33
Hának, Péter, 2
Harrower, Molly (1906–1999), 158
Hart, B. I., 84
Hausdorff, Felix (1868–1942), 27
Hecke, Erich (1887–1947), 22
Heims, Steve J., 2, 10, 11, 35, 38, 63, 65, 67–69, 81, 88, 90, 92, 140, 141, 153, 158, 160, 175
Heisenberg, Werner (1901–1976), 32, 33, 35, 37
Hellinger, Ernst (1883–1950), 22
Helmholtz, Hermann (1821–1899), 152
Herglotz, Gustav (1881–1953), 22
Hersh, Reuben, 4, 5
Hertz, Heinrich (1857–1899), 152
Heyting, Arend (1989–1980), 29
Hilbert, David (1852–1943), 6–8, 14, 15, 17, 19, 21–35, 37, 39, 49, 51, 53, 54, 58–60, 65, 66, 77, 78, 81, 125, 143, 154, 155, 167–169, 173
Hitch, Charles, 113
Hitler, Adolf (1889–1945), 11, 15, 78, 89
Horthy, Miklós (1868–1957), 10
Horváth, Zoltán, 2,
Hounshell, David, 107, 114

Hughes, Agatha C., 106
Hughes, Thomas P., 83, 105, 106, 109–115, 134
Hutchinson, George E. (1903–1991), 158

Impagliazzo, John, 9, 12, 68, 175
Ingrao, Bruna, 71, 72, 122, 123, 125
Israel, Giorgio, ix, x, 5, 6, 51, 54–56, 61, 62, 66, 68, 71, 72, 80, 122, 123, 125

Jammer, Max, 32
Jászi, Oszkar (1875–1957), 2, 11
John-Steiner, Vera, 4, 5
Jones, Thomas, 77
Jordan, Pascual (1902–1980), 32, 35

Kaldor, Nicholas (1908–1986), 11, 13, 46, 126
Kalmár, László (1900–1976), 5, 7, 8, 46
Kann, Jakab, 1, 2
Kann Neumann, Margit, 1
Kantorovic, Leonid V. (1912–1986), 117
Kármán, Theodore (von) (1881–1963), 7, 11, 22, 85
Károlyi, Mihály, 10
Kay, Lily, 141, 160
Kelvin, Lord (Thomson, William) (1829–1907), 152
Kemeny, John (1928–1992), 163
Kennedy, John F. (1917–1963), 113
Kennedy, John L. (1913–1984), 108
Kent, Robert H. (1886–1961), 52, 84
Kepler, Johannes (1571–1630), ix, x
Kerékjártó, Béla (1898–1946), 5
Kevles, Daniel J., 83
Killian, James R. (1904–1988), 117
Klausewitz, Karl (von) (1780–1831) 38
Klein, Felix (1849–1925), 19–24, 65, 76
Klein, Judith, 112, 140
Kline, Ronald, 83
Klüver, Heinrich (1897–1979), 158
Koebe, Paul (1882–1945), 22
Koestler, Arthur (1905–1983), 11, 12
Kolmogorov, Andrej N. (1903–1987), 117
König, Dénes (1884–1944), 7, 8, 46
König, Gyula (1849–1913) , 3, 4, 7, 14, 28, 46
König, Hans, 147
Koopmans, Tjalling C. (1910–1985), 135
Kövesi, Mariette, 15, 173
Koyré, Alexandre (1882–1964), 103
Kronecker, Leopold (1823–1891), 20, 25, 26
Kuhn, Harold W. (1925–), 126, 131, 133, 137, 165, 175
Kuhn, Thomas S. (1922–1996), 8
Kun, Béla (1886–1930), 10, 125, 173
Kürschák, József (1864–1933), 3, 14
Kutta, Martin W. (1867–1944), 150

Landau, Edmund (1877–1938), 22
Laplace, Pierre-Simon (1749–1827), 64
Lax, Peter D. (1926), 68
Lazarsfeld, Paul (1901–1976), 158
Lebesgue, Henri Léon (1875–1941), 37, 73
Leibniz, Gottfried Wilhelm (1646–1716), 6
Leijonhufvud, Axel (1933), 47
Leijonhufvud, Earlene, 47
Leonard, Robert J., 45, 107, 125, 130, 135
Leontief, Vassily (1906–1999), 136
Leslie, Stephen W., 115
Levi-Civita, Tullio (1873–1941), 32
Levin, M. R., 112
Lewy, Hans (1904–1988), 22, 151
Lie, Marius Sophus (1842–1899), 81
Lindbergh, Charles (1902–1974), 112

Lorente de No, Rafael (1902–1990), 158
Lotka, Alfred J. (1880–1949), 66, 161
Luce, R. Duncan, 133
Lucertini, Mario, 112
Lukács, György (1885–1971), 11

Mach, Ernst (1838–1916), 13, 29, 63, 124
MacNamara, Robert, 113
Mannheim, Karl (1893–1947), 11
Marschak, Jacob (1898–1977), 47
Maschke, Heinrich (1853–1908), 72
Mashaal, Maurice, 54
Masotto, Guido, 55
Mauchly, John W. (1907–1980), 96
Maupertuis, Pierre-Louis Moreau de (1698–1759), 62
Maxwell, James Clerk (1831–1879), 132, 157
McCagg, W. O., 1
McCarthy, John (1927–), 162
McCarthy, Joseph (1908–1957), 91
McCulloch, Warren (1898–1969), 141–143, 158, 159, 161, 163
Mead, Margaret (1901–1978), 158
Menger, Carl (1840–1921), 125
Menger, Karl (1902–1985), 13, 125, 126–129
Millán Gasca, Ana Maria, 55, 112, 135, 138
Mindell, David A., 112
Minkowski, Hermann (1864–1909), 22, 32
Mirowski, Philip, 31, 45, 47, 49, 51, 130, 131, 140
Moore, Eliakim H. (1862–1932), 76
Moravia, Sergio, x
Morgenstern, Oskar (1902–1977), 46, 47, 83, 84, 108, 128–133, 135–137, 174
Moy, Timothy, 83
Murray, Francis J. (1911–1996), 81

Nasar, Sylvia, 94, 130
Nash, John F. (1928), 51, 94, 108, 130–133
Nastasi, Pietro, 5, 80
Nelson, Horace, 71
Neumann Whitman, Marina (von), 12, 15, 17, 83, 146, 173
Neumann, Mihály, 2
Neumann, Miklós, see Vonneuman Nicholas A.
Neumann, Miksa (1873–1928), 1, 2, 9
Neurath, Otto (1882–1945), 13, 124, 125, 134
Newton, Isaac (1642–1727), ix, 6, 61–63, 66, 132, 147, 167, 169
Nicolò, Fernando, 112
Noether, Emmy (1882–1935), 22, 34, 53, 81
Noether, Max (1844–1921), 22
Nordheim, Lothar W. (1899–1985), 29, 35, 173

Oppenheimer, Robert J. (1904–1967), 15, 86, 88, 90, 91
Ortvay, Rudolf (1885–1945), 15, 16, 141, 157, 158, 160
Ostrowski, Alexander M. (1893–1986), 22

Pál, Jenö, see Wigner Eugene P.
Papp, K., 36
Pareto, Vilfredo (1848–1923), 112, 118, 123, 124, 130
Parshall, Karen H., 54, 75, 76
Pauli, Wolfgang (1900–1958), 65
Peano, Giuseppe (1858–1932), 25
Phillips, Norman, 153
Pickering, Andy, 140
Pitts, Walter (1923–1969), 141–143, 159, 161, 163
Planck, Max (1858–1947), 33, 61
Pleshakov, Constantine, 88
Plücker, Julius (1801–1868), 19, 20

Poincaré, Henri (1854–1912), 4, 13, 20, 23, 27, 30, 32, 59, 61, 71
Poincaré, Raymond, 5
Polanyi, Michael (1891–1976), 47
Pólya, György (1887–1985), 5, 7, 11, 13
Poundstone, William, 94
Prandtl, Ludwig (1875–1963), 22
Prigogine, Ilya (1917–2003), 61
Prüfer, Heinz (1896–1934), 14

Quarles, Donald A. (1894–1959), 91
Quesnay, François (1694–1774), xi, 72

Raiffa, Howard, 133
Ramo, Simon (1913), 111, 113, 115
Rashevsky, Nicholas (1899–1972), 163
Rátz, László (1863–1930), 3, 4
Rayleigh, Lord (Strutt, John W.) (1849–1919), 148, 152
Rédei, Miklos, 78, 81, 84, 117, 141, 155, 157, 160, 176
Rees, Mina (1902–1997), 87, 107
Reichelderfer, Francis W. (1895–1983), 152
Reingold, Nathan, 80
Reynolds, Osborne (1842–1912), 148
Richard, Jules (1862–1956), 26
Richardson, Lewis F. (1881–1953), 152, 153
Riemann, Georg Friedrich Bernhard (1826–1866), 20, 148
Riesz, Frigyes (1880–1956), 4–6, 12, 14, 34, 37, 39
Riesz, Marcel (1886–1969), 5, 7, 11
Roosevelt, Franklin D., 87
Rosenblueth, Arthur (1900–1970), 73, 140
Rossby, Carl Gustav (1898–1957), 152
Rossi, Arcangelo, 32
Rowe, David, 5, 19, 21, 66, 75, 76
Rozenblit, Marscha L., 1
Runge, Carl D. (1856–1927), 22, 85, 147, 150
Russell, Bertrand (1872–1970), 26–28, 58, 141
Rutherford, Ernest (1871–1937), 32

Samuelson, Paul (1915), 132
Schlesinger, Karl (1889–1938), 125, 127
Schlick, Moritz (1882–1936), 13
Schmidt, Erhard (1876–1959), 6, 13, 14, 35
Schriever, Bernard (1910–2005), 111, 113
Schrödinger, Ernst (1887–1961), 33, 35, 37, 160
Schumpeter, Joseph (1883–1950), 132
Schwarzschild, Karl (1873–1916), 22
Scorza, Gaetano (1876–1939), 125
Segrè, Emilio (1905–1989), 68
Shannon, Claude E. (1916–2001), 139, 156, 157, 160, 162, 164, 175
Shubik, Martin J. (1926), 130
Simon, Herbert A. (1916–2001), 135, 168
Singer, Isaac M., 9, 12, 68, 175
Smith, John K., 83
Snow, Charles P. (1905–1980), 121
Somenzi, Vittorio (1918–2003), 141
Sommerfeld, Arnold (1868–1951), 22
Stalin, Josif V. (1878–1953), 87, 88, 91
Staub, Hans, 68
Steiner, Jakob (1796–1863), 20
Steinitz, Ernst (1871–1928), 23, 27
Stibitz, George R. (1904–1995), 85, 86
Stöltzner, Michael, 81
Stone, Marshall H. (1903–1989), 81
Strauss, Lewis L. (1896–1974), 89–91, 118, 119
Sylvester, James J. (1814–1897), 76

Szász, Ottó (1884–1952), 5
Szegö, Gábor (1895–1985), 5, 7
Szénássy, Barna, 4, 7
Szilard, Leo (1898–1964), 11, 13, 36, 47, 88, 157, 159, 160

Taub, Abraham H. (1911–1999), 104, 176
Teller, Edward (1908–2003), 7, 11, 68, 89–91, 111
Todd, John A. (1908–1994), 85
Toeplitz, Otto (1881–1940), 22
Townshend, Aaron, 96
Truman, Harry (1884–1972), 87–90
Tucker, Albert W. (1905–1995), 126, 133, 135, 137, 165, 175
Turing, Alan (1912–1954), 143, 159, 161, 164

Ulam, Stanislaw M. (1909–1984), 11, 48, 81, 84, 89, 151, 162, 175

Valmos, T, 176
Van der Mark, Jan, 66
Van der Pol, Balthasar L. (1889–1959), 66, 139
Van Hove, Leon, 81
Veblen, Oswald (1880–1960), 15, 16, 52, 75–80, 82, 83, 85, 92, 116
Ville, Jean (1910–1988), 44
Volterra, Vito (1860–1940), 6, 55
Vonneuman, Nicholas A., 2, 175

Waerden, Bartel L. (van der) (1903–1996), 53
Wald, Abraham (1902–1950), 11, 125–127, 135
Walras, Léon (1834–1910), 46, 71, 123, 126, 130
Watson-Watt, Robert A. (1892–1973), 134
Weaver, Warren (1894–1978), 86, 107, 139, 143
Weber, Heinrich Martin (1842–1913), 23
Weber, Wilhelm Eduard (1804–1891) 114
Weintraub, E. Roy, 13, 47, 125, 126
Weyl, Hermann (1885–1955), 13–15, 22, 28, 58, 59, 77, 79
Whitehead, Alfred N. (1861–1947), 26, 141
Whitman, Robert, 17
Wicksell, Knut (1851–1926), 46
Widmann, Heinz, 79
Wiechert, Johann Emil (1861–1928), 22
Wiener, Norbert (1894–1964), 15, 22, 35, 37, 71–74, 81, 118, 139–141, 144, 153, 158–160
Wiesner, Jerome (1915–1994), 113
Wigner, Eugene P. (1902–1995), 7, 8, 11, 13, 29, 37, 47
Williams, John, 107
Wittgenstein, Ludwig (1889–1951), 13, 57, 128
Wooldridge, Dean E. (1913–1985), 111, 113, 115

Zach, R., 29
Zadeh, Lotfi A., 163
Zauberman, Alfred, 163
Zermelo, Ernst (1871–1953), 14, 22, 26, 27, 40, 41, 46
Zubok, Vladislav, 88
Zuse, Konrad (1910–1995), 85
Zworykin, Vladimir K. (1889–1982), 152